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THE MATHEMATICAL GAZETTE

EDITED BY
T. A. A. BROADBENT, M.A.

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THE TEACHING OF DIFFERENTIALS.

BY E. G. PHILLIPS, M.A., M.Sc.

MOST mathematicians are aware of the unhappy controversy between Newton and Leibniz about the priority of the discovery of the Infinitesimal Calculus*. This controversy lasted from 1707 to 1716, and, as Rouse Ball writes, "it occupies a place in the scientific history of the early years of the eighteenth century quite disproportionate to its true importance". It is, however, remarkable that this controversy is to a large extent responsible for the fact that even to-day much of the teaching of Elementary Calculus in this country is given in an unsatisfactory manner. The notation of differentials was invented by Leibniz in 1675, at about the same time as Newton invented his "fluxions". The bitterness of the aftermath of that unhappy controversy, however, resulted in the almost complete disappearance of the *differential* from English mathematics.

In order to deal most satisfactorily with the differentiability of functions of more than one variable, it is necessary for the student to acquire a knowledge of differentials. Frequently it happens that students whose early training has been on the lines of many of the existing elementary text-books on the Differential Calculus come up to the University never even having heard of a *differential*! It would be of the greatest possible assistance to those who are responsible for the later teaching of the Calculus if the schoolmasters taught the subject from the differential standpoint right from the start. I propose very briefly to indicate how this may be done.

One of the chief difficulties lies in the looseness of the terminology adopted in many of the elementary books. The terms "derivative" and "differential coefficient" are generally regarded as synonymous and interchangeable; but for a proper understanding of the differential a clear distinction must be made between these two terms.

The *derivative* is defined as follows. Let $y=f(x)$ be a finite single-valued function of x in a given interval (a, b), and let x be any point in this interval. If x be given a (positive or negative) increment $\Delta x=h$, then the corresponding increment of y, which we denote by Δy, is $f(x+h)-f(x)$.

The ratio of these increments is

$$\frac{\Delta y}{\Delta x}=\frac{f(x+h)-f(x)}{h}. \qquad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1)$$

If this ratio tends to a definite limit as h tends to zero, this limit is called the *derivative* of $f(x)$ at the point x, and it is usually denoted by $f'(x)$. Other notations which may be used for it are y', $D_x y$ or Dy. At this point it is

* See, for example, Rouse Ball's *History of Mathematics* (1912), pp. 356-362

essential to emphasise that the notation $\frac{dy}{dx}$ should *not* be used to denote the limit of the ratio (1).

From the definition of the derivative we easily deduce the two theorems (i) *If $f(x)$ is a constant its derivative is zero*, and (ii) *If $f(x)=x$ its derivative is unity*.

We next define *differentiability*. A function $f(x)$ is said to be *differentiable* at the point x, if it is finite and determinate in the neighbourhood of this point and if, when x is given the increment Δx which may be assigned arbitrarily, the increment Δy can be expressed in the form

$$\Delta y = A\Delta x + \epsilon\Delta x \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(2)$$

where A is independent of Δx and $\epsilon \to 0$ as $\Delta x \to 0$.

In this case the first term on the right-hand side of (2) is called the *differential* of y and it is denoted by dy (or by df). Thus $dy = A\Delta x$, and so

$$\Delta y = dy + \epsilon\Delta x.$$

By making $\Delta x \to 0$ we deduce from (2) that

$$\lim \frac{\Delta y}{\Delta x} = A,$$

and hence that, if $f(x)$ is differentiable, $f'(x)$ exists and has a finite and definite value A. It also follows from the definition of the derivative that if $f'(x)$ has a definite value A, the equation (2) holds. If the function $y=f(x)$ is differentiable we have therefore

$$dy = f'(x)\Delta x \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(3)$$

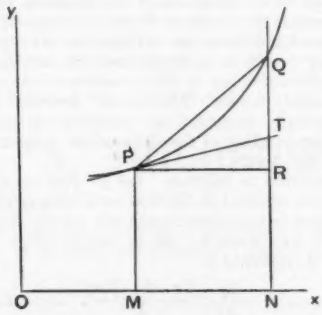
so that the differential of a function is the product of its derivative and an (arbitrary) increment Δx of the independent variable x. Now suppose that $f(x)=x$; we have seen that $f'(x)=1$ and (3) becomes $dx=\Delta x$. Hence the differential of the *independent* variable is the same as the (arbitrary) increment of that variable. Equation (3) now becomes $dy = f'(x)dx$,

or, on dividing by dx,

$$\frac{dy}{dx} = f'(x) \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(4)$$

It has therefore been proved that the derivative of a function $f(x)$ is the ratio of the differential of the function and the differential of the variable.

The geometrical meaning of the differential is at once clear from the figure.



If P and Q are the points (x, y) $(x+\Delta x, y+\Delta y)$ on the curve $y=f(x)$, the derivative of $f(x)$ is the slope of the tangent to the curve at P, and so

$$f'(x) = \tan \hat{TPR} = RT/PR = RT/MN.$$

Now by equation (4) above, $\frac{dy}{dx}=f'(x)$, and since $MN=\Delta x$ (which for the *independent* variable is the same as dx), we have $dy=RT$ while $\Delta y=RQ$. Clearly therefore Δy and dy are not the same, for $\Delta y-dy=TQ$.

From the point of view of the differential notation, the statement so often made in elementary teaching, that dy/dx is not a ratio of two numbers but a symbol denoting the operation* $\frac{d}{dx}(y)$ is most misleading. If the subject is carefully taught on the lines indicated above, the reasons for making this statement no longer arise, and it should be deliberately avoided. Another bad flaw in elementary teaching arises out of this: when the notation dy/dx is used for the *derivative*, and the student has been carefully warned that it is *not* a ratio, at a later stage an equation of the form $dy=g(x)dx$ is often written, although it has no meaning unless dy and dx have been subsequently defined. Without defining differentials it is not allowable even to write $ds^2=dx^2+dy^2$.

The distinction between derivatives and differential coefficients is of vital importance when the above ideas are extended to functions of more than one variable. The function $u=u(x, y, z)$ of the three independent variables x, y, z, is said to be differentiable at the point (x, y, z) if it possesses a determinate value in the neighbourhood of this point, and if

$$\Delta u=A\Delta x+B\Delta y+C\Delta z+\epsilon\rho \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(5)$$

where $\rho=|\Delta x|+|\Delta y|+|\Delta x|$, $\epsilon\to 0$ as $\rho\to 0$, and A, B, C are independent of Δx, Δy and Δz.

The limit (when it exists) of the incrementary ratio

$$\frac{u(x+\Delta x, y, z)-u(x, y, z)}{\Delta x}$$

as x tends to zero is defined to be the *partial derivative* of u with respect to x and it is written $\frac{\partial u}{\partial x}$.

In (5), if $\Delta y=\Delta z=0$, then $\Delta u=A\Delta x+\epsilon|\Delta x|$

and, on dividing by Δx and taking the limit as $\Delta x\to 0$ we get

$$\frac{\partial u}{\partial x}=A.$$

Similarly $$\frac{\partial u}{\partial y}=B, \quad \frac{\partial u}{\partial z}=C.$$

Thus, when the function u is differentiable, the partial derivatives are also differential coefficients (coefficients of the differentials dx, dy and dz). By putting $u=x$, $u=y$, $u=z$ respectively it easily follows that $dx=\Delta x$, $dy=\Delta y$, $dz=\Delta z$. The differential of the dependent variable, du, is defined to be

$$\frac{\partial u}{\partial x}dx+\frac{\partial u}{\partial y}dy+\frac{\partial u}{\partial z}dz.$$

If the function u is differentiable at the point (x, y, z) the partial derivatives of u with respect to x, y and z certainly exist and are finite at this point; but it can be shown by examples that the partial derivatives may exist at a point although the function is not differentiable at that point: in other words, the partial derivatives need not always be differential coefficients. Since these results belong to a later stage in the teaching of the subject they will not be further discussed here, but enough has been said to emphasise the desirability of introducing the concept of a differential right at the commencement of the teaching of the Differential Calculus. E. G. PHILLIPS.

* See Edwards, *Differential Calculus for Beginners* (1912), p. 17.

PROBABILITY AND ITS APPLICATIONS.

By Prof. H. T. H. Piaggio, M.A., D.Sc.

"The theory of probabilities", said Laplace, "is really nothing more than common sense reduced to arithmetic". Later writers, however, seem of opinion that much of the theory is founded, not on common sense, but on uncommon nonsense. Thus Chrystal, referring to what are often called inverse probabilities, advised actuaries "much as they justly respected Laplace, not to air his weaknesses in their annual examinations. The indiscretions of a great man should be quietly allowed to be forgotten". More recently Keynes has denounced Laplace and his successors in the strongest terms, and has endeavoured "to discredit the mathematical charlatanry by which, for a hundred years past, the basis of theoretical statistics has been greatly undermined". The purpose of the present article is to give some account of the theoretical foundations of the calculus of probabilities and to indicate some of its applications to statistics, telephony and biology. The earlier and abstract part of the article will be most easily grasped by reading it in conjunction with the later and concrete part, starting from the paragraph headed Weldon's Dice Data. A short list of recent text-books will be found at the end.

Definitions.—The usual definition of probability divides all the possibilities into a finite number of *equally-likely* cases, and takes the ratio of the number of favourable cases to the total number. Laplace and Stumpf take events to be equally likely when there is no reason to suppose them to be different. This *Principle of Insufficient Reason* would lead us to attribute the probability of $\frac{1}{2}$ to every event about which we knew nothing, which is easily shown to lead to paradoxes (cf. Fry, p. 117).

Von Kries and others rely upon the Principle of Cogent Reason, which takes events as equally likely only when, after a careful examination, they appear to be similar in all essential respects. However, both principles have the cardinal defect of failing in all except the simplest cases. The six faces of a real die (with small holes in the faces to mark the number of pips) are only approximately similar, while there is not even an approximate *a priori* groundwork for predicting the probable expectation of life, or the probable number of telephone calls. On this account many writers (*e.g.* Ellis, Cournot, Venn and Chrystal) have relied upon a statistical definition of probability, as the ratio of the number of favourable cases to the total number (supposed large) of an actual series of trials. It was soon realised that this ratio was not constant, so the definition was altered by taking the limit of the ratio as the number of trials tended to infinity. But does this limit actually exist? Some claim that this can be proved, but they base their opinion on the properties of a Bernoulli distribution (defined below), forgetting that this is only a hypothetical distribution and not an actual one. Others (*e.g.* Coolidge) postulate the existence of this limit as one of the fundamental empirical assumptions. But as Fry points out (p. 90), to postulate that a limit exists is to restrict the fluctuation of the ratio in a way that is inconsistent with the idea of independence; in other words, to demand that after a certain number of trials, long runs of either favourable or unfavourable cases must never occur, which we cannot do unless we control the events. Keynes, who regards probability as relating to judgments and not to events, and as essentially depending on the premises, maintains (p. 34) that except in a special type of case no numerical treatment is possible.

The Uniformity Theory.—The theory of probability that appeals most to the present writer is a combination of some of Venn's and Lexis' statistical ideas with the methods of Hilbert's work on the foundations of geometry.

I believe that these concepts are at the back of the mind of many statisticians, but I have never seen them fully expressed, though Borel (pp. 1-3) gives the abstract side of the theory and Bohlmann applies it to life assurance. Coolidge says that he would like to see this method developed, but that it is of no real importance; a view from which I strongly dissent. In my opinion, the probability of which mathematicians and statisticians treat is primarily a measure of uniformity which underlies certain (but not all) series of events. This series of events may be a contemplated one lying in the future, as the proposed tossing of certain dice, or may lie wholly in the past and be fully known (*e.g.* Weldon's dice data considered below), in which case it is clear that we have diverged considerably from the original concept of probability as a rational measure of expectation of an unknown event. It is a pity that a different word is not available for the new concept; R. A. Fisher has already appropriated the term *likelihood* for another measure of expectation, different from probability both in Keynes' sense and in mine.

Abstract Definition.—The first part of our treatment is to build up an abstract science deducing the mathematical consequences of postulating that to each event of a series there corresponds a positive number p, its probability, which is undefined except indirectly in that it is to obey certain laws. The Addition Law is the postulate that if a number of events are mutually exclusive, the total probability of one or the other of them occurring is the sum of the separate probabilities. If we make the convention that the probability of an event which is certain to happen is 1, the Addition Law shows that, taking q as the probability of an event not happening, $p+q=1$. The second postulate is the Multiplication Law, that the probability of two independent events both occurring is the product of their separate probabilities, with a similar law for dependent events, except that the second factor is now the *conditional* probability of the second event when the first is certain. A third postulate, that equally likely events (if any such can be found) have equal probabilities, enables us to take over all the usual elementary results concerning games of chance. With each event may be associated a number x (which may be positive or negative).

If we have N events, the arithmetic mean $\bar{x}$ of the x's and their standard deviation σ are defined by

$$\bar{x}=\Sigma x/N\,; \quad \sigma=\{\Sigma(x-\bar{x})^2/N\}^{\frac{1}{2}}.$$

In the calculation of σ it is often convenient to use the identity

$$\Sigma(x-\bar{x})^2=\Sigma x^2-2\bar{x}(\Sigma x)+N\bar{x}^2=\Sigma x^2-2\bar{x}(N\bar{x})+N\bar{x}^2=\Sigma x^2-N\bar{x}^2.$$

Tchebycheff's Theorem.—It will be noticed that if λ is any positive number exceeding 1, not more than N/λ^2 of these x's can differ numerically from $\bar{x}$ by more than $\lambda\sigma$, for if so these alone would make $\Sigma(x-\bar{x})^2$ exceed

$$(N/\lambda^2)(\lambda\sigma)^2=N\sigma^2,$$

which is the total for all the x's. This is Sheppard's form of the proof of Tchebycheff's theorem.

Bernoulli Distributions.—We can now consider certain hypothetical distributions of events corresponding to special assumptions concerning p. The simplest is the *Bernoulli* or *binomial* distribution based on a constant p. In n trials the probability of exactly r favourable cases or successes (implying $n-r$ failures) is, by the Multiplication Law, p^rq^{n-r} for every possible way of combining r successes and $(n-r)$ failures. The number of such ways is ${}^nC_r$, hence, by the Addition Law, the probability required is ${}^nC_rp^rq^{n-r}$, which is the $(r+1)$th term of the expansion by the binomial theorem of $(q+p)^n$. If N groups of n trials, of which each group could show a number x of successes between zero and n inclusive, were distributed according to this law,

so that r successes occurred exactly ${}^nC_r p^r q^{n-r} N$ times, the arithmetic mean and standard deviation would be given by

$$N\bar{x} = \Sigma {}^nC_r p^r q^{n-r} N r$$

and

$$N\sigma^2 = \Sigma {}^nC_r p^r q^{n-r} N r^2 - N\bar{x}^2,$$

so that

$$\bar{x} = \Sigma \frac{n!}{r!(n-r)!} p^r q^{n-r} r = np \Sigma \frac{(n-1)!}{(r-1)!(n-r)!} p^{r-1} q^{n-r}$$

$$= np(q+p)^{n-1} = np,$$

and

$$\sigma^2 + \bar{x}^2 = np \Sigma \frac{(n-1)!}{(r-1)!(n-r)!} p^{r-1} q^{n-r} (\overline{r-1}+1)$$

$$= np \left\{ (n-1) p \Sigma \frac{(n-2)!}{(r-2)!(n-r)!} p^{r-2} q^{n-r} + \Sigma \frac{(n-1)!}{(r-1)!(n-r)!} p^{r-1} q^{n-r} \right\}$$

$$= np\{(n-1)\, p\, (q+p)^{n-2} + (q+p)^{n-1}\}$$

$$= np\{(n-1)\, p + 1\},$$

hence

$$\sigma^2 = np\{(n-1)\, p + 1\} - (np)^2 = np\,(1-p) = npq.$$

Poisson's Exponential.—There are two specially interesting limiting cases of Bernoulli distributions. If we let p approach zero and n approach infinity in such a way that the product np is constant, $=\epsilon$ say, then

$${}^nC_r p^r q^{n-r} = \frac{n(n-1)(n-2)\ldots(n-r+1)}{r!} \left(\frac{\epsilon}{n}\right)^r \left(1 - \frac{\epsilon}{n}\right)^{n-r}$$

$$= \left\{\left(1-\frac{1}{n}\right)\left(1-\frac{2}{n}\right)\ldots\left(1-\frac{r-1}{n}\right)\left(1-\frac{\epsilon}{n}\right)^{-r}\right\} \left\{\left(1-\frac{\epsilon}{n}\right)^n\right\} \left\{\frac{\epsilon^r}{r!}\right\}$$

$$\to e^{-\epsilon} \frac{\epsilon^r}{r!}.$$

This result is known as Poisson's Exponential Limit, or The Law of Small Numbers. As the finite binomial series is now replaced by an infinite (exponential) one, it is not safe to assume that the new $\bar{x}$ and σ are the limits of the old ones, giving ϵ and $\epsilon^{\frac{1}{2}}$ respectively.

However, these results are true and the sum of the series is still 1, as can be easily verified.

Normal Law.—Let us represent the terms of the symmetrical Bernoulli distribution $(\frac{1}{2}+\frac{1}{2})^n$ by $(n+1)$ ordinates at equal distances c apart and situated symmetrically about the axis of y. The end (x_r, y_r) of the rth ordinate is given by

$$x_r = (r - \tfrac{1}{2}n - 1)c, \quad y_r = {}^nC_{r-1} (\tfrac{1}{2})^n.$$

Hence if m be the gradient of the line joining (x_r, y_r) to (x_{r+1}, y_{r+1}), and (x, y) be its mid-point, and σ be the standard deviation

$$\left\{\frac{\Sigma y_r x_r^2}{\Sigma y_r}\right\}^{\frac{1}{2}},$$

then

$$\frac{m}{y} = \frac{y_{r+1} - y_r}{\frac{1}{2}(y_{r+1} + y_r)\, c} = \frac{2({}^nC_r - {}^nC_{r-1})}{c({}^nC_r + {}^nC_{r-1})}$$

$$= \frac{2\left(\dfrac{n-r+1}{r} - 1\right)}{c\left(\dfrac{n-r+1}{r} + 1\right)} = \frac{2(n-2r+1)}{c(n+1)};$$

$$x = \tfrac{1}{2}(x_{r+1} + x_r) = \tfrac{1}{2}c(2r - n - 1);$$

and $\sigma = c\,(npq)^{\frac{1}{2}} = \frac{1}{2}cn^{\frac{1}{2}}$ (by work nearly the same as that in the paragraph headed Bernoulli Distributions, except for the additional factor c); hence

$$\frac{m}{y} = -\frac{4x}{c^2(n+1)} = -\frac{4x}{4\sigma^2 + c^2}.$$

Now let n approach infinity in such a way that σ remains finite, so that c approaches zero. The polygon joining the ends of the ordinates becomes the continuous curve whose differential equation is

$$\frac{1}{y}\frac{dy}{dx} = -\frac{x}{\sigma^2},$$

giving

$$y = y_0 e^{-x^2/2\sigma^2}.$$

Hence if a set of statistics containing a large number N of separate results is distributed as shown by the terms of the expansion of $N(\frac{1}{2}+\frac{1}{2})^n$, when n is large, the number $N_x^{x+\delta x}$ of results whose deviation from the mean (or "error") lies between x and $x+\delta x$ is approximately proportional to

$$\delta x \,.\, e^{-x^2/2\sigma^2},$$

say k times this expression. Since all errors must lie between $-\infty$ and $+\infty$, we get k from the equation

$$N = \int_{-\infty}^{\infty} k e^{-x^2/2\sigma^2} dx = 2^{\frac{1}{2}} k\sigma \int_{-\infty}^{\infty} e^{-(x/2^{\frac{1}{2}}\sigma)^2} d(x/2^{\frac{1}{2}}\sigma) = 2^{\frac{1}{2}} k\sigma\pi^{\frac{1}{2}},$$

giving finally

$$N_x^{x+\delta x} = \frac{N\delta x}{\sigma(2\pi)^{\frac{1}{2}}} e^{-x^2/2\sigma^2}.$$

This is called the Normal Law of Error.

Elaborate tables based on the Normal Law are available, and it is found that the probabilities of an error numerically exceeding $0{\cdot}6745\sigma$ and 3σ are respectively 0·5 and 0·0027. Physicists and others call $0{\cdot}6745\sigma$ the Probable Error, but artillery manuals speak of the interval $\bar{x} \pm 0{\cdot}6745\sigma$ as the 50 per cent. zone. They go on to say that the 100 per cent. zone, in which all shots will fall, is four times the 50 per cent. zone. This is equivalent to saying that the probability of an error numerically exceeding $2{\cdot}698\sigma$ is zero. According to the Normal Law, no error, however great, can have a probability zero, but of course this cannot be true of even the most unskilled of gunners. To take a more frivolous example: in playing pitch and toss grave suspicion should be entertained of an opponent who in 100 tosses contrives to turn up more than 65 heads. Here $p = q = \frac{1}{2}$, $\bar{x} = 50$, $n = 100$, $\sigma = (npq)^{\frac{1}{2}} = 5$, $\bar{x} + 3\sigma = 65$.

Although we have considered Poisson's Exponential and the Normal Law as approximations, it should be stated that they are also exact forms corresponding to certain assumptions rather different from those underlying the Bernoulli distribution. For instance (Fry, pp. 220-228) the Poisson Law can be obtained from the simple hypothesis that the events are distributed "individually and collectively at random", which means roughly that they happen quite independently of each other, as is presumably the case of a man using the telephone without knowing what the other subscribers are doing, or a cavalry horse kicking its rider without pre-arrangement with the other horses of the regiment. It may be applied to the calculation of demands for service in general, *e.g.* of manufactured articles in a large shop, of certain occurrences at an electrical generating station, as well as to physical phenomena like the emission of β rays.

Poisson Distributions.—There is another distribution associated with the name of Poisson, differing from the Bernoulli one by having variable p's, and thus replacing $N(q+p)^n$ by $N(q_1+p_1)(q_2+p_2)\ldots(q_n+p_n)$. If we calculate

$\bar{x}$ and σ, we can make x the same as before by taking p to be the arithmetic mean of $p_1, p_2, \ldots p_n$, but the σ will come less than before. Such a distribution is said to have *subnormal* dispersion. This corresponds to the hypothetical result of tossing N rapidly wearing pennies n times each in which the probability of a head is different for each toss of the same penny, but does not vary from one penny to another.

Lexis Distribution.—In contrast to this we have a Lexis distribution, corresponding to the hypothetical result of tossing n durable but badly made pennies N times each, for which the probability of a head is constant for each toss of the same penny, but varies from one penny to another. It may be proved that $\bar{x}$ can be made the same as in the Bernoulli case by taking p as the arithmetic mean of $p_1, p_2, \ldots p_n$, but the ratio of the new standard deviation to the old (called the *Lexis Ratio*) is now greater than unity, giving *supernormal* dispersion. The researches of Lexis were concerned with the ratio of male to female births, and with rates of mortality, but they appear to be applicable to a much greater range. It should be noticed that Tchebycheff's theorem can be applied to any of these distributions. Thus taking $\lambda = 3$, the proportion of results deviating from the mean by more than 3σ cannot exceed $1/9 = 0{\cdot}1111$. However, as this result is so much greater than the corresponding 0·0027 found from the Normal Law, which generally holds at any rate approximately, Tchebycheff's criterion, in spite of its theoretical advantages, is of very little use in practice. Closer inequalities have been found by Pearson, Camp, Guldberg, Meidel and Narumi by imposing mild restrictions on the nature of the distribution function, but still leaving it sufficiently general to be useful in actual problems of statistics.

Weldon's Dice Data.—The distributions considered above show the nature of the first or abstract part of our treatment. Of course they can be supplemented by others whenever the necessity arises, as in fact it does in telephone problems. We shall now pass to the second part, the fitting of our hypothetical distributions to actual ones. As a first example consider Weldon's Dice Data. By tossing 12 dice 26,306 times and counting either a 5 or a 6 as a success he obtained the results given in column 2 of the following table:

Col. 1.	Col. 2.	Col. 3.	Col. 4.
No. of successes.	Frequency observed.	Frequency calculated from $p = \frac{1}{3}$.	Frequency calculated from $p = 0{\cdot}3377$.
0	185	202·75	187·38
1	1,149	1,216·50	1,146·51
2	3,265	3,345·37	3,215·24
3	5,475	5,575·61	5,464·70
4	6,114	6,272·56	6,269·35
5	5,194	5,108·05	5,144·65
6	3,067	2,927·20	3,042·54
7	1,331	1,254·51	1,329·73
8	403	392·04	423·76
9	105	87·12	96·03
10	14	13·07	14·69
11	4	1·19	1·36
12	0	0·05	0·06
—	26,306	—	—

Column 3 is obtained by assuming that all six faces are equally likely to come uppermost, giving $p = \frac{2}{6} = \frac{1}{3}$, and then computing the terms of the expansion of $26{,}306\,(\frac{2}{3} + \frac{1}{3})^{12}$. The discrepancy between columns 2 and 3 is

greater than could be desired. If, however, we take p for each die to have the value 0·3377, which is obtained by dividing the mean

$$\{(0\times185)+(1\times1149)+(2\times3265)+\ldots+(12\times0)\}\div26{,}306=4\cdot0523$$

by 12, we get the figures in column 4, which agree much better. An even better fit could have been obtained by assuming twelve different values of p, one for each die, thus giving a Lexis distribution, but this would need a great deal of labour. Of course we must not expect our hypothetical distribution to fit the real one exactly, and we must remember that by choice of several variables we are really unduly forcing this fit. Considerable attention is now being paid to tests of Goodness of Fit. Denoting the observed and calculated frequency in any class by f and g respectively, Pearson denotes

$$\Sigma(f-g)^2/g \text{ by } \chi^2.$$

This is closely connected with the measure of dispersion which Lexis denotes by Q, for $Q=\chi^2/n$, where n is the number of classes in which the frequencies may be taken arbitrarily. Pearson gave criteria to determine for any particular values of n and χ^2, whether the hypothesis as to distribution can be regarded as reasonable. The accuracy of his formulae has been considered by R. A. Fisher, "Student", Sheppard and Yule. These results are of great practical use, but their theoretical basis is still insecure. It apparently makes more use of the Normal Law than has yet been justified.

Cavalry Accidents.—As a second example we take Von Bortkiewicz's data of the cavalrymen killed by kicks of their horses, from the records of ten German army corps for twenty years:

Col. 1.	Col. 2.	Col. 3.
No. of deaths.	Frequency observed.	Frequency calculated from $\epsilon=0\cdot61$.
0	109	108·67
1	65	66·29
2	22	20·22
3	3	4·11
4	1	0·63
5	0	0·08
6	0	0·01
—	200	—

As the number of men is very large and the chance of any one being killed very small, it is reasonable to try to fit a Poisson Exponential Series with a typical term $200e^{-\epsilon}\frac{\epsilon^r}{r!}$, giving an arithmetic mean

$$\epsilon=\{(1\times65)+(2\times22)+(3\times3)+(4\times1)\}\div200=0\cdot61.$$

The fit appears to be excellent, though Keynes (p. 405) says that it is a paradoxical result really latent in the Lexis measure of stability, and showing its unreliability. It is natural to suspect a result which seems to attribute a high degree of discipline and education to horses, even those of a highly disciplined and educated nation like Germany, but really no such assumption is made. What is assumed is that the tendency to become violent is more or less constant in a collection of horses selected, trained and fed according to fixed rules, and that when this tendency is indulged in it is without regard to the behaviour of other horses. "Student" has shown that the distribution of certain biological data (number of yeast cells in 400 squares into which a square millimetre was divided) agreed very well indeed with the Poisson

Series. However, when we come to telephone calls, the agreement is not so good, possibly because users of the telephone, though largely unaccountable for their actions, are not as fully so as savage horses or yeast germs.

Telephones.—We shall now explain how telephone engineers calculate the probable proportion of lost calls, when there are ν trunk lines available and the average number of these required is ϵ. The probability that at any time exactly r lines are required is, by Poisson's Exponential Law, $e^{-\epsilon}\frac{\epsilon^r}{r!}$. Also if the average duration of a call is T minutes, the probability that this call began in any particular short interval dt minutes (contained in the larger interval T minutes) is $\frac{dt}{T}$. This result assumes that one particular line is being used; the corresponding probability for a call beginning on any of the lines is got by multiplying by the average number of lines required, giving $\frac{\epsilon dt}{T}$. Hence the probability that in the interval dt exactly r lines are required and that then another call comes is, by the Multiplication Law, $\left(e^{-\epsilon}\frac{\epsilon^r}{r!}\right)\left(\frac{\epsilon dt}{T}\right)$. If r exceeds $(\nu-1)$ when this happens, the call is lost, so the total probability of a call being lost in the interval is

$$\left(\frac{\epsilon dt}{T}\right)e^{-\epsilon}\sum_{\nu}^{\infty}\frac{\epsilon^r}{r!}.$$

Dividing this by the probability $\frac{\epsilon dt}{T}$ of a call coming, we get the probable proportion of lost calls as $e^{-\epsilon}\sum_{\nu}^{\infty}\frac{\epsilon^r}{r!}$.

It may be observed that the proof of this formula considers a call as lost if not immediately successful, though it assumes, among other things, that if a subscriber finds a line engaged he continues trying to get through, spending in this way the same time that he would have spent if the call had been successful at first. This is an extreme assumption. The other extreme is to assume that if he finds the line engaged he goes away immediately. The real truth lies between. In Fry (Chap. X) will be found no less than six sets of carefully tabulated assumptions, with the resulting mathematical formulae, supplemented in some cases by elaborate numerical tables and graphs. Perhaps the Poisson formulae (given above) are the simplest and the Engset the most accurate, while the Erlang give a good compromise. Considerable mathematical skill has been expended in transforming the formulae into forms suitable for calculation, and we are left with the feeling that if mathematics and the theory of probability die out in our schools and universities, they will still be kept alive by telephone engineers.

Kinetic Theory of Gases.—Nothing has been said of inverse probabilities, geometrical probability, and the kinetic theory of gases. There is one element common to all these, in that they cannot be applied without more or less arbitrary assumptions. Geometrical probability is little more than a mathematical toy, though the assumptions in it appear to be similar to those made in the kinetic theory of gases, but in the latter case they are (to a certain extent) justified by their results. A theory of probability based on the idea of uniformity ought to find its application to a collection of molecules, and perhaps the present results have really a firmer basis than has yet been revealed. In the words of Keynes (p. 428), "If the contemporary doctrines of Biology and Physics remain tenable, we may have a remarkable, if undeserved, justification of some of the methods of the traditional Calculus of Probabilities".

SHORT LIST OF RECENT BOOKS ON PROBABILITY AND STATISTICS.

T. C. Fry: *Probability and its Engineering Uses.* 30s. 1928. (Macmillan.)

A. Fisher: *The Mathematical Theory of Probabilities.* 21s. 2nd ed. 1928. (Macmillan.)

[A. Fisher is an American Actuary and Telegraph Engineer; he should not be confused with the English statistician, R. A. Fisher, author of *Statistical Methods for Research Workers*—15s. 3rd ed. 1930. (Oliver and Boyd)—a book dealing especially with recent advances in the theory of the reliability of small samples.]

L. G. Du Pasquier: *Le Calcul des Probabilités, son évolution mathématique et philosophique.* 10s. 1926. (Hermann.)

E. Borel: *Principes et Formules classiques du Calcul des Probabilités.* 18 francs. 1925. (Gauthier-Villars.)

H. L. Rietz: *Handbook of Mathematical Statistics.* 18s. 1924. (Houghton Mifflin.)

J. M. Keynes: *A Treatise on Probability.* 18s. 1921. (Macmillan.)

[This book deals with Probability more as a branch of Philosophy and Logic than of Mathematics.]

It should be pointed out that all the books in this list are of a fairly advanced nature, and presuppose an acquaintance with the ordinary treatment of probability (as in Smith's or Hall and Knight's *Algebra*), or, in the case of statistics, of first courses such as that of Caradog Jones.

The application of Probability and Statistics to Psychology has made great progress in recent years. This may form the subject of a future article.

University College, Nottingham. H. T. H. Piaggio.

GLEANINGS FAR AND NEAR.

808. Sir,—Mr. Snowden, in his efforts to be sanguine, is reported to have said, in his speech yesterday at Accrington, "It has been said that if you could induce the Chinese to wear shirts of British cotton cloth only 2 in. longer there would not be enough spindles and looms in Lancashire to supply the extra demand in normal times." He might have chosen a happier illustration, for unfortunately the Chinese do not wear shirts, and 2 in. longer than nothing is still nothing.—Yours, &c.,

October 22. Pessimist.

The Times, Oct. 24, 1930. The fallacy was foreseen.

"Take some more tea," the March Hare said to Alice, very earnestly.

"I've had nothing yet," Alice replied in an offended tone, "so I can't take more."

"You mean you can't take *less*," said the Hatter: "it's very easy to take *more* than nothing." [Per Prof. E. H. Neville.]

809. To be wholly devoted to some intellectual exercise is to have succeeded in life; and perhaps only in law and the higher mathematics may this devotion be maintained, suffice to itself without reaction, and find continual rewards without excitement.—R. L. Stevenson, *Weir of Hermiston*, chap. ii. [Per Mr. James Buchanan.]

810. Alas! a natural incapacity for teaching, finished by keeping terms at Cambridge, where there are able mathematicians, and butter is sold by the yard, is not apparently the medium through which Christian doctrine will distil as welcome dew on withered souls.—Geo. Eliot, *Scenes of Clerical Life: The Sad Fortunes of the Rev. Amos Barton*, chap. ii. [Per Mr. James Buchanan.]

THE VALUE OF EXACTNESS.

By Dr. Cyril Norwood.*

Now that many months have elapsed and the wheel has come full circle and the day has come when I must address the London Branch of the Mathematical Association as their unworthy President of the year, I find that the bubble of my conceit is pricked and all my resources are exposed as empty air. My qualifications for my task of this afternoon are indeed hardly existent. I did, indeed, begin my school life at the tender age of 13½ as a junior mathematical scholar, a distinction which I owed to a rather exceptional memory; I began to founder when I set forth on the sea of geometrical conics. I foundered entirely in analytical conics, and at the age of 16, the earliest age at which we were allowed to specialise, I went to the chief mathematical master and asked him leave to drop the subject. "Understand", he said to me, "that if you drop this subject now, I will never teach you mathematics again". This dreadful prospect did not deter me, for it was exactly what I was hoping to secure; I gained my own way, and have indeed never regretted it until this afternoon, when I find myself in your presence, and ill-provided for my ordeal.

I have chosen as my subject the value of exactness, and let me begin my theme by recalling to your memories an incident of Greek history, not perhaps so well known as it should be. It was a theory of that great philosopher, Plato, that the perfect state would never be seen on this earth until either philosophers became kings, or kings became philosophers. Unlike most theorists, he received an invitation to convert his theory into practice, for there was at Syracuse a young and absolute tyrant, the younger Dionysius, who expressed the desire to be moulded by Plato into a state of fitness to rule and to direct for the best the fortunes of the Greek race in the Western Mediterranean. "Plato", writes Grote, "could hardly fail to conceive hopes from the warmth of his first reception. One of the royal carriages met him at his landing and conveyed him to his lodging. Dionysius offered a sacrifice of thanksgiving to the gods for his safe arrival. The banquets in the acropolis became distinguished for their plainness and sobriety. Never had Dionysius been so gentle in answering suitors or transacting public business. He began immediately to take lessons in geometry from Plato". Now, that is the reason why I recall this story to your minds, that the first subject which Plato —surely one of the greatest thinkers that the world has produced—the first subject which he thought was desirable for the instruction of an absolute ruler was geometry. There then followed what must surely be considered one of the most amazing scenes that has ever been witnessed in a royal court. "Everyone around him was suddenly smitten with a taste for geometry, so that floors were all spread with sand and nothing was to be seen except triangles and other figures inscribed upon it, with expositors and a listening crowd around them."

The experiment did not work,, but we need not mind about that. The question which I start by asking is what made Plato take this extraordinary course, and I think that the answer is that he held that the first qualification for a politician, for a man entrusted with the task of directing the lives of others was capacity for clear thought, an ability trained to argue inexorably from cause to effect. In the dialogue called the Meno, Plato is writing about the value of true opinions, which are not yet knowledge, and he says that they are like the images of Daedalus, which require to be fastened in order to keep them, and if they are not fastened they will play truant and run away. He says that this is an illustration of the nature of true opinions, that they abide with us, they are beautiful and fruitful, but they run away out of the human soul and do not remain long, and therefore they are not of much value until they are fastened by the tie of the cause. That, he says, is why knowledge

* Presidential Address to the London Branch of the Mathematical Association.

is more honourable and excellent than true opinion, because fastened by a chain. I believe, therefore, that Plato held mathematics to be a necessary part of education, just because it brings clearly into the mind the inexorable relation of cause and effect, the logical necessity that if this and that were so, then the other must be so. He speaks somewhere of three studies suitable for free man—arithmetic is one of them; the measurement of length, surface, and depth is the second; and the third has to do with the revolution of the stars in relation to one another. But lest you should be carried away by the flattering idea that Plato held mathematicians to be the intellectual salt of the earth, I must add that he held all these studies to be elementary and merely preparatory, suitable for an ignorant Dionysius, but not for the philosopher and the advanced thinker. "You surely could not regard the skilled mathematician as a didactician", he makes Glaucon say in the *Republic*. "Assuredly not", Socrates answers, "I have hardly ever known a mathematician who was capable of reasoning." And this seems an odd thing for Plato to have written when we remember how he treated Dionysius.

The fact is (for I do not propose to embark on the consideration of the Platonic Theory of Ideas, Dialectic, and the Idea of the Good) that what is always at the back of Plato's mind is the difference between knowledge and opinion, and that was at the back of my mind too when I proposed trying to speak to you of the value of exactness. There was such a state as not knowing and half knowing, which was the lot of the many in Plato's day, and there was such a state as knowing, which was the lot of very few, and the result of the rigorous application of methodical enquiry. And the same is true of the world to-day. Let me read one more passage, and then I will have done with Plato's words, though not with his line of thought. "We seem to have discovered", says Socrates, "that the many ideas which the multitude entertain about the beautiful and about all other things are tossing about in some region which is half way between pure being and pure not being. And since we have before agreed that anything of this kind which we might find was to be described as matter of opinion, and not as matter of knowledge, a sort of intermediate flux caught and defined by the intermediate faculty, then the condition is that those who see the many beautiful, and who yet neither see absolute beauty, nor can follow any guide who leads the way thither, who see the many just, and not absolute justice, and the like, such persons may be said to have opinions but not knowledge." Now my thesis is, that there were never so many as there are at the present time who can be described as being in this state, never so many who are quarter educated and half educated, never so many who are incapable of appreciating cause and effect, the nature of convincing evidence, what is meant by fact and what is meant by truth. And it seems to me that there was therefore never a time when the necessity for universal elementary mathematical study was more apparent, or the fruit of it likely to be more valuable. At the same time I do not want to be thought that I consider this elementary study as sufficient in itself to bring people from the flux of opinion to the certainty of knowledge. It is but a beginning, in the right way.

I was much impressed by some remarks made to me shortly after the war by a distinguished scientist, whose lot it had been in conjunction with other scientists to work with a committee of which the remaining members were all politicians. He told me that the difficulty of that committee was for the one side to arrive at a common understanding with the other. To the scientists there was a problem, but it was governed by facts, and until these facts were correctly ascertained it did not seem to be possible to make any progress, or even to understand what the problem was until it could be correctly stated. But to the politician there was a problem indeed, but the facts were of much less importance: for a fact was to them not a fact, but something which was susceptible of different repercussions according to the needs and the policy of

the moment. If it was an awkward fact, it could be explained away; if it were a favourable fact, it could be thrown into a high light, and made to dominate the situation. He told me that this difference between the two sides was so marked that it was almost impossible for them to work together.

We are now set free from the stern exigencies of a time of war, but we were never more deeply divided than now into the few who know and the many who do not. We have also further developed a system of government in this country in which preponderant power is placed in the hands of the ignorant many, and therefore in the hands of those who can most successfully persuade the ignorant. I will, with your leave, take three classes of the community who appear to me to be inexact, dealers in seeming and not being, and therefore dangerous guides, and I will compare them with three other types that seem to me to depend upon the value of exactness for use in the efficiency of their work.

I naturally begin with the politician, as we know him in this democratic age, either here or in the United States or France. He sets out to praise his own party and to vilify or to bring into contempt the other party or parties; his aim is avowedly not truth but success at the polls and the holding of power. Trained in this way, with a mind instinctively prepared to give a colour to any set of circumstances, and to turn them to party advantage, he has never learned to think of facts as an exact statement of things as they are, but always as a set of factors which can receive a particular kind of representation. Such a man, we will say, arrives at the seat of power, bound by all sorts of such promises and hampered by all kinds of voluntary and involuntary misrepresentations. There he is faced by sets of particular circumstances in which he is called on to do the best that he can for his country, as he may often quite honestly desire to do. He will not succeed unless he tries really to arrive at the exact facts, but his whole habit of mind is inimical to this: it is inexact. Consequently we slip along from one muddle to another.

Let us take practical examples of this, and I shall choose my examples not at all in the spirit of the party politician. I will take first the great problem of unemployment. It is a matter for our country of life and death. But the treatment which it has received at the hands of politicians has been for Labour to cry up and down the land from north to south that the incompetence of the Conservatives had landed Britain in great and increasing unemployment, which they if they were in power could conjure away in a few months; and then, again, for the Conservatives to cry up and down the land from north to south that the still greater incompetence of Labour had landed Britain in unemployment twice as severe. They have sought to give to their fellow-citizens inexact impressions in order to add to party capital and prestige. Yet I submit that thinking men must agree that we are here in the presence of something which purely party politicians can neither cause nor cure, and which is therefore not a party question at all. It is a problem in which it is the first duty of trained economists to state the ascertained facts and to describe the factors which govern the problem—and their next duty, to indicate the cure, if cure there be, or at any rate what alterations can be attempted. It would then be the duty of all citizens of good will to co-operate, proceeding on a basis of exact knowledge as far as it can be gained. Instead of that, we are merely deafened by the clamour of quacks asking us to swallow their patent medicines in faith, or, to change the metaphor, to take various leaps into the dark. It can only be a happy accident if on this method we win through to safety.

I will take another instance, and this time from a subject in which we are most of us interested, the subject of education. There was a document called the Hadow Report which may or may not have been the last word in educational wisdom, but was at any rate prepared by men and women who analysed a situation, studied the facts, and prescribed a reform. That reform was to be a change in the organisation of our education, an increase of school

life by a year, and the provision of a course of instruction for all children from 11 to 15. It would have been natural after this to do two or three things, first, to ascertain as between the main political parties whether there would be any chance of substantial agreement, and then to study the cost, which perhaps, if long prepared for, need not have been great; next, if no overwhelming opposition appeared, to set forth what as a matter of fact should be the course or the courses to be studied by the children, so that it might be known what this education was actually to be; thirdly, on this basis, to see where the teachers were to come from, and within what period of years they could be recruited and trained. That, I submit to you, would be the course followed by those who were trained in the value of exactness. On the course that has actually been followed I prefer not to comment, but I do not think I am wrong in saying that at the present moment no one quite knows either what the children are to be taught, or who is to teach them, or where the teaching is to take place.

The second class that I would take are perhaps even worse offenders than the politicians, and I refer to the journalists. There was a time when a considerable number of journalists set themselves to ascertain facts and to present a true account of them. They were careful to separate comment from the narrative of facts. But the journalists who have this high ideal of their profession are usually fewer to-day, for they find it hard to live and the journals which welcome the sort of work which they can do are fewer still. For the present state of things, I think that we have to hold responsible the wide diffusion of a very imperfect education, which has brought into existence for every thousand who can read barely one who desires to think, and still fewer who *can* think, when they desire to do so.

We have to consider the effects upon a community of all that is implied by "stunt" journalism, which relies almost wholely on methods of mass suggestion. I take comfort from the undoubted fact that these efforts not infrequently defeat themselves, since the intellectual level of the population, low as it is, is not so low as some of our newspapers think that it is. But even so, the whole journalistic process is disastrous to true intellectual judgment and wholely hostile to truth. For truth does not come into it; her vital forces are concealed or ignored, and only those facts are made public which support or illustrate the impression or opinion which it is sought to broadcast.

Quite apart from the whole conscious process of misrepresentation which goes on in the Press to-day, varying as it does, from reports of "tendencious" character and wilful make-up and presentation of news all the way to deliberate falsification, I do not think that journalists as a class have any conception of the value of exactness, or any training which will enable them to appreciate it. They deal almost wholly in words, and usually well-worn familiar words which have ceased to bear any exact meaning unless you make a hard intellectual effort to fix them down, and their object is the formation or the division of opinion. We all know that it is very seldom that two admittedly honest witnesses will give the same account of a simple succession of events which have passed before their eyes and of which they are trying to give a true account. If this is the normal weakness of human minds, what addition to truth is likely to be made by the account of a journalist who is told to make "copy" of the same set of events. His idea of language is usually always something after this fashion, that this is the sort of thing you say and write in this set of circumstances, and this is the sort of thing which you say and write in that other set of circumstances. It is not so much due to moral obliquity as to bad training and the influence of commercialism on the whole journalistic profession that I have never known even an approximately true and exact account given of any set of circumstances of which I have had personal knowledge and which happened unfortunately to be of any value as journalistic copy. As soon as it is felt that reported incidents must be made to conform to accepted types which the people will understand and appreciate,

or are capable of being made into a sensation, then truth is left undisturbed at the bottom of her well, and no attempt is made to disturb the waters which cover her. In a word, my charge against modern journalists is that the words they use are inexact, their thoughts are inexact, and their purpose is inexact, insomuch as it is not what it professes to be. Their output provides almost the entire mental nourishment that a very large section of our population finds opportunity to absorb. The third class which I will use as illustrations of inexactness are clergymen, ministers and teachers, against whom I have by no means as good a ground as I have against the politicians and journalists, but I do charge them with being guilty of too frequent inexactitudes. Mr. H. G. Wells has put this in a far stronger way than I could think just or fair, when he somewhere describes schoolmasters as dull dogs of invincible mediocrity, mostly afraid of anything unorthodox in politics, ethics or religion. It is a charge by no means true of all of them, nor is it a fair charge to bring against their real leaders in any generation. But conventional orthodoxy is a serious charge, and who shall say that it is entirely undeserved? But it leads to much inexactness. The charge against the clergy that they think one thing in their studies and preach another in the pulpit is not entirely undeserved. The doctrine of the Trinity is indisputably difficult, and it requires a hard mental effort to grasp what the Catholic Church is trying to safeguard—the Unity of the Godhead. How many of the clergy make a conscious effort so to preach in the pulpit that the ordinary person does not get the impression that there is by no means one God but Three. How many, though they know that the fourth gospel is an interpretation and not an exact narrative of the Life and Words of Jesus Christ, hesitate to use it at times as if the words were the *ipsissima verba* of Jesus? How many give the impression that they are really facing the facts of modern life, and not approaching them with an attitude which is already predetermined? To all these rhetorical questions I should answer, a certain number, but not many. I do not think that clergymen ask themselves as often as they should, "What do I really mean by that sentence? Am I saying what I really think, and do I really think, or am I only saying what I think I am expected to say?" And exactly the same kind of weakness attacks the schoolmaster, who often says and does the things which he thinks he is expected to say and do, and would be hard put to it to give a reason. We are all very familiar with the phenomenon known as the "closed" mind and with that other phenomenon like the character in *Middlemarch*, who refused to follow out a train of thought to its conclusion because he felt that it would carry him too far. All this is due to a lack of what I am calling the value of exactness, by which I mean not being content with anything less than the truth, so far as we are able to discover it.

I promised to set against those classes which I have chosen for criticism those other classes for whom I hold that it may be fairly claimed that they have a sense of the value of exactness, though I would enter the warning that I do not mean my three classes to be more than typical; they are not exhaustive. First I would put the doctors. Their diagnosis is based on a careful examination of all the factors in the case in so far as they are capable of being discovered, and their treatment is based on the discovered result of similar treatment applied to similar cases, and placed upon record. They are constantly extending the bounds of their knowledge by experiment and observation, and they are constantly altering their methods of treatment in the light of that increased knowledge. They do not neglect the temperature of 102 because they would prefer it to be 98·7, and they do not ignore a subcutaneous rash because the patient would look nicer without it. In other words, they do not do as the politicians do. Another class that I would take is that of our engineers. When they set out to build a tunnel through a mountain range they do not set out south-easterly from one side and north-westerly from the other, and drill their holes and hope for the best. They take

exact bearings, and proceed with the utmost mathematical exactness; in due course, with mathematical exactness they meet. They start to build some great bridge over a vast span, and their work goes on month after month. Little by little, all of it exactly planned, exactly measured, and exactly placed until at last the final piece swings into position and locks the whole. They don't trust to luck to meeting somewhere over mid-channel when they can throw something across which will more or less do. In other words, neither in tunnel-driving nor in bridge-building do they set out as journalists do. And the final class which I would take as men of an approximation to our clergymen and teachers are farmers and agriculturalists. For at their best they know the value of exactness. They know that they must study exactly the value of the soil and the average probabilities of the climate, the comparative values of seeds, and the recorded results of methods of culture. In other words, they know that scientific cultivation will pay and unscientific will not. But they can also be like the schoolmasters, dull dogs of invincible mediocrity, who are mortally afraid of anything unorthodox in methods of agriculture, and farmers of this type do not progress, and in the long run they do not endure. It is, indeed, very tempting to follow out this parallel between farmers and schoolmasters, those who cultivate the soil and those who cultivate the mind, and observe the many curious parallels that exist between them; but I have been too long already. I will leave you to reflect in relation to this topic of the value of exactness on the comparative merits of politicians, journalists and teachers on the one hand, and doctors, engineers and farmers on the other.

Let us turn to another danger which threatens all of us, even if we have the best will in the world to be exact, and that arises from the very nature of words, the medium of human speech. We inherit words which were coined by other men in past and very different generations; they have changed since those days, and they are always changing. We make a certain number of new words, but these too change as they pass into general currency. Many of these words have been familiar to us all our lives, and we cannot dismiss them and their assumptions from our minds. But many of them express relations of terms to which nothing exactly or nothing at all in the kingdom of fact corresponds. The world has often been led away by a word to which no distinct meaning could be attached. I will try to be precise; the sort of words that I have in mind are abstractions like "authority", "equality", "utility", "liberty", "pleasure", "experience", "consciousness", "chance", "substance", "matter", "atom"; even such a common ordinary word as "free" has as many colours and qualities as a chameleon—free love, free thought, free trade, free fight, free beer, free education, free art, free verse, free stories: I do not know how long you could not make the list if you set yourself conscientiously to write down all the senses and phrases in which the word is used. But the upshot of it is this, that just because of the inexactness of the only instrument of communication that we can use, words, it is necessary for us all to make the utmost efforts to be exact, to be jealously correct in our use of terms, and to see that we have as exact a meaning as we can arrive at in the sentences which we employ. It is hard enough to enter into the full meaning of a contemporary, because of this changing chameleon-like character of words, and because those words carry different connotations in the minds of different individuals. To enter fully into the meaning of a past generation is still more difficult, and into that of a remote generation is I believe impossible.

You mathematicians will say that here you have the advantage, and that two means two, three means three, oddness is oddness, and even is even. And there I am in agreement with you. You alone can be entirely exact, and therefore, as it seems to me, your studies are likely to carry us furthest in the search for truth, even though we may think that for beings limited as we are ultimate Truth will remain unknowable. Many people are now talking about that interesting and popular book by Sir James Jeans, *The Mysterious Universe*,

and quote his remarks such as that "the essential fact is simply that all the pictures that science now draws of nature, and which alone seem capable of according with observational fact are mathematical pictures", or that "from the intrinsic evidence of his creation, the Great Architect of the Universe now begins to appear as a pure mathematician". It is easy to make a certain amount of fun of this, and to decry the possibility of ever offering whole-hearted worship to the square root of -1, or to any mathematical formula. But I hold that things are working out as they are because the concepts in which mathematics deals are pure abstractions, its processes the processes of pure thought. Words are all of them bound up with the sensible, warm notions of our material lives, and they have the temporary inexact character of all the things of sense. For that reason the bloom of each philosophy built with words passes away, and the "eternal" truths of a Descartes or a Hegel do not last for many generations. It is not so with the philosophy built on numbers, even though it has been left to our present generation to discover how true this is—dreamed of by Plato, thought of by Galileo, and made more actual by Einstein. But beware of the interpretation of these results by words. That is the answer, so far as an answer can be given, to those who allege that when all is said and done, man is only creating God after his own image: to the mathematician He is the great Mathematician, to the biologist the great Biologist, to the artist the great Painter and Creator of Beauty. Only yesterday a letter to the *Times* protested that man is not what he sees, but what he feels, and quoted the wonderful words of Shelley:

"The desire of the moth for the star,
Of the night for the morrow,
The devotion of something afar,
From the sphere of our sorrow."

All that I would urge that mathematics, following abstract numbers and pure thought, and being as far removed as possible from things of sense, is likely to lead towards ultimate Truth more reliably than observation of material phenomena. But I do not for a moment suppose that mathematics can explain life in its fullness, or do other than omit many of the real values of life. I think that there is a good deal of confused thinking on this topic, and lest in my praise of the value of exactness I should seem myself to be blind to the fact that there are many spheres in which exactness is neither desirable nor possible, I would like to safeguard myself by developing my position a little further.

I will borrow from Canon Streeter's book on *Reality* an illustration which briefly and clearly sets out what I have in mind. He is arguing in the passage to which I refer that Science and Religion each give you a representation of reality which without the other is incomplete, and he uses this illuminating analogy. "I wish", he says, "to explain something about Venice to a friend who has never been there, and there are readily accessible both the plan of the City in Baedeker's guide, and Turner's famous picture. Which will be most useful? It depends entirely upon the immediate purpose of our conversation. If my sole object is to show him the exact position of an hotel which I recommend, the plan is what I want, and the picture is worthless. If I wish to prove that Venice is well worth a visit, or if my aim is to suggest to him an attitude of mind, which will enable him to get the most benefit from a visit, the picture is the thing. And if I wish to convey to him the best idea I can of the place as a whole, I shall use both plan and picture."

To my mind it is quite true to say that mathematics and science answer to the plan of the analogy, and religion and art to the picture.

"Life," to quote Shelley again,

"Life like a dome of many coloured glass,
Stains the white radiance of eternity,
Until death tramples it to fragments",

and in this transitory life the wonder and the beauty of the Universe are as much a fact of reality as the reason whose laws operate the whole. We cannot look for the same exactness on this side of reality, and we may be tempted to say that it has no place and no value at all, while we lose ourselves in mysticism, or in religious and aesthetic contemplation of the infinite and unknowable. And yet I am bold to think that even in the region of the spiritual values there is such a thing as exactness which we should know, respect and seek to attain.

Let us call these spiritual values Truth, Beauty and Goodness, and I will take it that I have sufficiently set forth the value of exactness in the search for Truth. Beauty is the domain of art. How far can poetry, for instance, be exact?

There comes into my mind the story of a mathematician who wrote to Alfred Tennyson after the publication of one of his poems, and objected to a couple of lines very strongly.

"Every moment dies a man,
Every moment one is born."

It is obvious, he wrote, that if this were the case the population of the world would be static and stationary, whereas the census proves that it is manifestly increasing. I submit to you that you would observe the laws of verse and at the same time not offend against observed Truth if in future editions you wrote:

"Every moment dies a man,
And one and a sixteenth is born."

I think we shall agree that this kind of exactness has no place in poetry. But there is a kind of exactness which has a very real place, so that poetry has been even defined as "the right words in the right places". It may be an inadequate definition, but I need not argue that here. Poetry is the perfect expression of some universal thought and therefore must be exact: the right, the exact word, makes the verse memorable for ever. One can but seek to show it by examples. "After life's fitful fever he sleeps well": put any word in place of fitful and it becomes commonplace. "Fitful" is aesthetically exact.

"A noise like of a hidden brook
In the leafy month of June,
That to the sleeping woods all night
Singeth in quiet tune."

Alter any word in that if you can: it is exact. Or that of Wordsworth,

"a sense sublime
Of something far more deeply interfused,
Whose dwelling is the light of setting suns".

All great poetry has this characteristic. Some call it felicity, but it is really exactness, the choice of the word that is felt to be right and quite inevitable.

It is the same with painting, sculpture and music. In painting there is exactness in the work of all the masters; and I do not mean merely that they draw well and truly, or produce exact delineations of figures and scenes. I mean that they so blend form and colour that they give with exactness the inner meaning, the emotion of reverence and awe that veils a Madonna and Child, the beauty of simplicity and the universality that illumines some Dutch subject—like a woman reading a letter by a window—or the strength of personality for good or evil that a great portrait painter makes to stand out from his canvas. The best work of the great masters of all schools never goes out of date, though it has not the imperishability of words, because it possesses somehow the exactness which gives universality.

Not only in painting but in sculpture also there is need of both kinds of exactness, the lesser and lower which consists in being accurate, the greater

and the higher which consists in being true. The one is the exactness of form and the other is exactness—I hope I do not stress the word unduly—exactness of spirit. So far as we can tell, in sculpture it has been given to one race, the Greeks, to reach to the full in their best work exactness in both senses, and to me the statue of Hermes by Praxiteles represents all that man can do. Set such a statue by the side of work by Epstein, or even by Rodin, and the greater exactness and truth of the earlier work will become manifest.

Music in its turn is governed by exact mathematical laws, and the mathematical and the musical are closely allied. But it has also the quality of rightness which appertains to all great art, whether it be some simple song from the Western Hebrides, or the Unfinished Symphony, a sea chanty or a fugue by Bach. In the interpretation of beauty in all its many forms exactness has value, but the difficulty has always been that once a race or a school has attained to perfect expression through its medium it establishes a tradition, and the tradition becomes a convention, and the life rapidly dies out of it. This is so in all the arts: each interpretation comes to perfection and loses its bloom. There is no absolute but only a relative truth, a truth relative to the knowledge and the emotions of a passing human generation: it is not given to their successors to reproduce the same perfection. Hence we see in our own day the dominance of revolt, the cult of the ugly instead of the beautiful, of the inexact for the true: bad colour, bad drawing, ugly moulding, distortions, inharmonious music, dissonant and senseless poetry—all of them false and quite temporary because they are all of them inexact. But they may be, as it were, the raw material out of which some master of coming generations will build up a new and exact interpretation of beauty, higher and more complex than the world has yet accomplished. Of one thing I am certain: it will have truth of form as well as truth of spirit. It will be no one-sided misrepresentation such as to-day we have to put up with while artists are feeling their way.

There remains the spiritual value of goodness, and here again exactness has its place. Indeed one whole system of ethics, that of Aristotle, has been built up on that exact sense of proportion which was the conspicuous quality of the Greeks. Every virtue, he argued, is found both in excess and in defect; it should be our object to cultivate the mean, the neither too much nor too little. Not for nothing was the inscription "Nothing too much" written over the temple of Delphi. Stinginess is wrong, and extravagance is wrong: there is something in between that is right.

What else do we mean when we speak of a well-balanced character? To the Greek, the finite was that with which he felt at home: the infinite made him uncomfortable. Kant was perhaps right when he criticised Aristotle for making a merely qualitative distinction between virtue and vice—as if you could go round and measure up moral characters with a tape measure.

You can of course resolve a statue into so much stone and bronze and the laws of proportion, and you can resolve a virtuous act into human passions and a quantitative law, but they are both rather destroyed than analysed in the process. Certainly I do not think that this kind of exactness is likely to be of great help to us in the pursuit of goodness, and it is noteworthy that there was none of this exact calculation in Him whom many regard as the Son of God, and nearly all as the greatest teacher of ethics. "When you have done all, then say you are an unprofitable servant." "Love your enemy and turn the other cheek." "Forgive unto seventy times seven." "Sell all that thou hast and give to the poor." There are a hundred such sayings that would have been an offence and a stumbling-block to a Greek moral philosopher. Yet we feel that there is somehow more truth in them than in the moral arithmetic of Aristotle: "High Heaven rejects the lore of nicely calculated less and more".

It will be obvious, accordingly, that I am afraid of introducing this conception of exactness into religion and ethics, lest we become formalists like

the Pharisees of old. Yet there is a framework of exactness within which our moral life is lived, and that is the system of law by which our life is conditioned. It is in the clear realisation of these laws that the value of exactness consists. And first I would place the law that nature never forgives, that consequences will work themselves to their end. Nature never argues, but always acts. There are communities to-day which are inclined to defy this law, and I am not sure that our own is not one of them. A community which lives at a higher standard than it can afford is a community which is defying a law of nature, and is inexact in its thinking, just as much as an individual who lives beyond his means. It is not only individuals but communities also who live on the principle of Mr. Micawber, always hoping for something to turn up. It is probable, again, that arms and the power of destruction have now come to so advanced a state of efficiency that another world war would mean the suicide of a civilisation. I hold that to be an exact and ascertained truth, but I wish that I could feel quite comfortable and sure that Europe is incapable of repeating the folly and blindness with which at the beginning of this century the nations committed themselves to the current which could only sweep them to a foreseen destruction. The fact that nature never forgives is more clearly seen in the case of individual lives, and it is known that intemperance, immorality and excess of any type are contrary to the laws of health and well-being. But there is no exactness in men's thoughts: they always think that they will be the particular exception that proves the rule.

The valuable contribution which mathematics and science have to make to the theory and practice of ethics is the conception of the immutability of law, which I repeat is the framework within which our human lives are set, and of which it is most desirable for every man to have exact knowledge. Yet poor human nature is on this matter woefully inexact, as witness many proverbs: "To cry for the moon", "Crying over spilt milk", "Locking the stable door when the horse is stolen", and all the metaphors from will-o'-the-wisps, bubbles, and rainbows, and gold that glitters. Biology and the science of hygiene have increasing information to impart as to the health of the body, all of which can be couched in the form of exact law: psychology has more and more to tell us that is reliable about the laws which govern the health of the mind. And there is the moral law itself, so that Kant may have had reason on his side when he said that "the moral law within me, and the starry heavens above me, most fill my soul with wonder". The value of exactness in our moral life is bound up in the conceptions of Law and Duty, and if these seem negative and restrictive, yet I would remind you that to the insight of the poet Wordsworth it did not seem that this is so:

> "Stern lawgiver! yet thou dost wear
> The Godhead's most benignant grace,
> Nor have we anything so fair
> As is the smile upon thy face.
> Flowers laugh before thee on their beds.
> And fragrance on thy footing treads:
> Thou dost preserve the stars from wrong
> And the most ancient heavens, through Thee, are fresh and strong."

I am well aware that all this talk of duty and the moral law is regarded now-a-days as old-fashioned, and that what I regard as the kingdom of law is by the antinomianism of the rebels of the present day regarded as the kingdom of opinion.

It is no doubt a good thing to question received tradition to its foundations, even if it be a matter of religion or morals. But the disastrous inexactness of a certain amount of present-day thought seems to me to be that there is no such thing as law in the moral sphere, but only thinking makes it so, and that the conception of virtue and the conception of sin are mere delusions imposed upon

mankind by the Church. I do not think that the experience of mankind will be lightly overthrown, or that the present phase of rebellion and irresponsible conduct is more than a passing phase, even though bad conduct has rarely been so open and unashamed. I regard our present phase as a combination of many circumstances. The disillusionment produced by the war, the weariness of convention, the transitional stage through which religion is passing, the dissatisfaction with worn-out types of achievement in poetry, painting, sculpture and music, great though they were in their own day, so that, as Matthew Arnold wrote, "we wander between two worlds, the one dead, the other powerless to be born". We are in the midst of the birth-pangs of a new age, things alter almost every month visibly before our eyes. We have witnessed what in brief may be called the emancipation of women, and the revolt of youth. We have seen Russia bound hand and foot by an oligarchy of the proletariat: we have seen tyrants enthroned and democracy failing. It is no wonder that many may think that all is a matter of opinion, and the individual the measure of all things, right and wrong being subjective and God a delusion. That to me is inexact thinking, but it is going on alike in the spheres of religion and morals, politics and economics: I have no doubt that God or the Nature of Things has sharp lessons in store for a stiff-necked generation. "I am inclined to agree with the Universe", said the lady to Carlyle, "Gad, Madam, you'd better!" replied the sage.

Exactness which is indeed the full knowledge of truth is indeed unattainable; we must do the best we may. But we have lights to steer by, and we need not be swept away by an eddying gust. The chief hope of the human communities of the future must lie in the degree to which the ordinary man can become a man of trained intellect, and of trained character who knows and respects the exactness which is truth. It may be that like the captives in Plato's cave, chained and facing the wall on which the firelight plays, we can only hope to see the shadows of the images cast on the wall by real objects somewhere behind us which in themselves we never see, yet for all that the objects which cast the shadows are real, and we have light enough to see by. Though we are passing through a time of journalism, demagogy, rebelliousness and self-seeking, I am not despondent. I look to clear thinking, real education and devotion to truth to save us, and for that reason have chosen to address to you whose duty it is to demonstrate what absolutely clear thinking means, this address, which I fear is already over long, on the "Value of Exactness".

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811. Taking $\frac{dy}{dx}$ of $\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$,

$$\frac{dy}{dx}=\frac{2x}{a^2}+\frac{2y}{b^2}\frac{dy}{dx}=0;$$

$$\therefore \frac{dy}{dx}=-\frac{b^2x}{a^2y}.$$ [From a script.]

812. "His course of instruction also included . . . advanced arithmetic, geometry, and algebra, in which he was fairly good, always saying that he found a certain poetry in the rule for the extraction of the cube-root, owing to its rhythm, and in some of the 'Miscellaneous Questions' of Walkingame. In applied mathematics he worked completely through Tate's *Mechanics* and Nesbitt's *Mensuration*." [This refers to his schooling, 1853-54.]—Florence Emily Hardy, *The Early Life of Thomas Hardy*, 1840-1891; ch. i., p. 31 (1928). [Per Mr. P. J. Harris.]

813. "Hardy makes a note that on June 25 [1880] they slept in Llanherne [the house he took in Wimborne] for the first time, and saw the new comet from the conservatory."—*Ibid.* ch. xi., p. 193. [Per Mr. P. J. Harris.]

MATHEMATICAL NOTES.

994. [D. 2. d. ; E. 1. c.] *Note on a Continued Fraction.*

In a recent paper,* in the course of proving a result stated by Ramanujan, I established the equivalence of the continued fraction

$$\frac{1}{x+}\,\frac{1^2}{x+}\,\frac{2^2}{x+}\,\frac{3^2}{x+}\cdots \qquad \text{.....(1)}$$

and the infinite series

$$2\left\{\frac{1}{x+1}-\frac{1}{x+3}+\frac{1}{x+5}-\ldots\right\}, \qquad \text{.....(2)}$$

by proving that each of them is equal to

$$\int_0^\infty \frac{e^{-xu}\,du}{\cosh u}.$$

An interesting proof of the equivalence of (1) and (2) is as follows. Stieltjes† has shown that

$$\frac{\Gamma(x-\frac{1}{2}a+\frac{1}{2})\Gamma(x+\frac{1}{2}a+\frac{3}{2})}{\Gamma(x+\frac{1}{2}a+\frac{1}{2})\Gamma(x-\frac{1}{2}a+\frac{3}{2})}-1$$
$$=\frac{2a}{(4x-a)+}\,\frac{1^2-a^2}{4x+}\,\frac{2^2-a^2}{4x+}\,\frac{3^2-a^2}{4x+}\cdots.$$

If we write x for $4x$, divide by a and take the limit as $a\to 0$, it follows that

$$\tfrac{1}{2}\left\{\psi\left(\frac{x+3}{4}\right)-\psi\left(\frac{x+1}{4}\right)\right\}=\frac{1}{x+}\,\frac{1^2}{x+}\,\frac{2^2}{x+}\,\frac{3^2}{x+}\cdots \qquad \text{.....(3)}$$

where $\psi(z)\equiv\dfrac{d}{dz}\log\Gamma(z)$.

From the known formula ‡

$$\psi(z)=-\gamma-\frac{1}{z}+z\sum_{n=1}^{\infty}\frac{1}{n(z+n)},$$

we deduce that

$$\psi(z)-\psi(z-\tfrac{1}{2})=\frac{1}{z-\frac{1}{2}}-\frac{1}{z}+\sum_{n=1}^{\infty}\left(\frac{1}{z+n-\frac{1}{2}}-\frac{1}{z+n}\right).$$

By writing $z=\frac{1}{4}(x+3)$ the left-hand side of (3) becomes

$$2\left\{\frac{1}{x+1}-\frac{1}{x+3}+\frac{1}{x+5}-\frac{1}{x+7}+\ldots\right\},$$

and the equivalence of (1) and (2) is established. E. G. PHILLIPS.

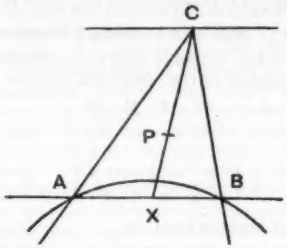
995. [K¹. 6. a.] *To find the conditions that the point $P(x', y')$ should be within the triangle whose sides are $L\equiv lx+my+n=0$ and*

$$S\equiv ax^2+2hxy+by^2+2gx+2fy+c=0.$$

(1) If $C(x_0, y_0)$ be the point of intersection of the lines $S=0$, the point P must lie between $L=0$ and the parallel to $L=0$ through C, *i.e.* $L-L_0=0$. The condition for this is that L' and $L'-L_0$ should have opposite signs, or that $L_0/L'>1$, *i.e.* $(Gl+Fm+C)/C(lx'+my'+n)>1$.

* Note on a problem of Ramanujan, *Journal London Math. Soc.* 4 (1929), 310.
† *Quart. J. of Math.* 25 (1891), 198.
‡ Whittaker and Watson, *Modern Analysis* (1920), 241.

(2) If CP meet AB in X, X must lie within any circle through AB (see figure).



The equation of such a circle is

$$T \equiv ax^2 + 2hxy + by^2 + 2gx + 2fy + c + (lx + my + n)(l'x + m'y + n')$$
$$\equiv \lambda(x^2 + y^2) + \text{linear terms} = 0.$$

Transposing the terms of the second degree, $lx + my$ must be a factor of $ax^2 + 2hxy + by^2 - \lambda(x^2 + y^2)$, whence, by the remainder theorem,

$$\lambda = (am^2 - 2hlm + bl^2)/(m^2 + l^2).$$

In substituting the coordinates of X in T, we note that X is on $L = 0$, and therefore the last term of T vanishes.

Since also for the point C, $X_0 = Y_0 = Z_0 = 0$, the equation of CP is any one of the consistent set $\frac{X}{X'} = \frac{Y}{Y'} = \frac{Z}{Z'} = \mu$; so that for a point (x, y) on CP,

$$S \equiv xX + yY + Z = \mu(xX' + yY' + Z')$$
$$\equiv \mu(x'X + y'Y + z'Z)$$
$$= \mu^2(x'X' + y'Y' + Z') \equiv \mu^2 S'.$$

Since, then, λ^2 and $l^2 + m^2$ are positive, the condition that the power of the point X for $T = 0$, *i.e.* T/λ, should be negative is that $S'/(am^2 - 2hlm + bl^2)$ should be negative. N. M. GIBBINS.

996. [A. 3. k.] *Graphical interpretation of the operations in the Theory of Equations in the case of the Cubic, leading to a simple investigation of the Nature of the Roots.*

Consider the general Cubic function

$$p_0x^3 + p_1x^2 + p_2x + p_3 = Y_0:$$

its graph cuts the Y-axis at 0, p_3.

Dividing by p_0, the form

$$F(x) \equiv x^3 + 3a_1x^2 + 3a_2x + a_3 = Y$$

may now be used, borrowing binomial numerical coefficients for ultimate convenience.

The corresponding effect on the graph is a proportional change of ordinates, with change of sign if p_0 is negative.

Substituting $x = X + h$,

$$(X + h)^3 + 3a_1(X + h)^2 + 3a_2(X + h) + a_3 = Y,$$

or $$X^3 + 3(h + a_1)X^2 + 3(h^2 + 2a_1h + a_2)X + h^3 + 3a_1h^2 + 3a_2h + a_3 = Y.$$

Choosing h so that the coefficient of the second term vanishes, *i.e.* $h = -a_1$, the expression becomes

$$X^3 + 3X(a_2 - a_1^2) + 2a_1^3 - 3a_1a_2 + a_3 = Y,$$

corresponding to a move of the Y-axis a_1 to the left.

The form is now $X^3 + 3HX + G = Y$, G being $F(-a_1)$, the ordinate in which the curve intersects the new Y-axis.

Draw a new $\parallel$ X-axis through this point,

$$X^3+3HX=Y-G=y.$$

Changing the sign of X merely changes sign of y, hence we have cross symmetry about these axes.

$y=0$ if $X=0$, $\pm\sqrt{-3H}$ (real if H $-^{ve}$).

Sign of y unlike $\left.\begin{matrix}\\ \text{like}\end{matrix}\right\}$ sign of X as X^2+3H $\begin{matrix}-^{ve},\\ +^{ve}.\end{matrix}$

$\frac{dy}{dX}=3(X^2+H)$, $+^{ve}$ for all values of X if H $+^{ve}$.

H $-^{ve}$, $\frac{dy}{dX}$ $-^{ve}$ if $X^2<-H$,

$+^{ve}$ „ $X^2>-H$,

0 „ $X^2=-H$.

$\frac{d^2y}{dX^2}=6X$ has sign of X.

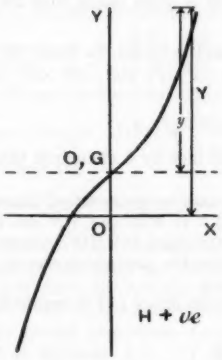


FIG. 1.

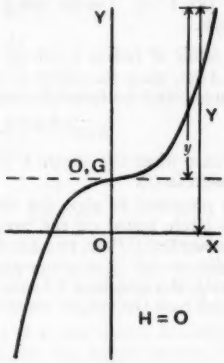


FIG. 2.

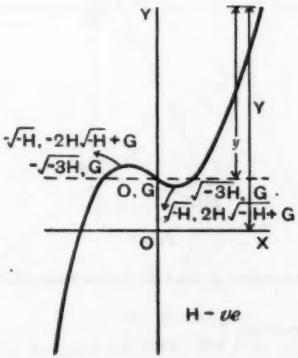


FIG. 3.

Obviously equation $X^3+3HX+G=0$

has always one real root of sign opposite to G.

Ordinate corresponding to $\sqrt{-H}$ on $X^3+3HX+G=Y$ is

$$(-H)^{\frac{3}{2}}+3H(-H)^{\frac{1}{2}}+G=2H(-H)^{\frac{1}{2}}+G$$

to $-\sqrt{-H}$

$$-(-H)^{\frac{3}{2}}-3H(-H)^{\frac{1}{2}}+G=-2H(-H)^{\frac{1}{2}}+G.$$

There are two additional different real roots of like sign if these ordinates differ in sign,

i.e. $[\pm 2H(-H)^{\frac{1}{2}})]^2>G^2$,

i.e. $4H^3+G^2$ −ve.

Coincident roots if $4H^3+G^2=0$.

A. HINCKLEY.

997. [K¹. 1. c.] *Circles which cut off chords of given length from the sides of a triangle.*

If a circle of radius x cut off chords of lengths $2l, 2m, 2n$ from the sides of the ΔABC, then its centre is at distances $\sqrt{x^2-l^2}$, $\sqrt{x^2-m^2}$, $\sqrt{x^2-n^2}$ from those sides, and we have the relation

$$a\sqrt{x^2-l^2}+b\sqrt{x^2-m^2}+c\sqrt{x^2-n^2}=2\Delta. \quad \ldots\ldots\ldots\ldots\ldots\ldots\text{(i)}$$

The case when the centre is outside can be met by a change of the appropriate sign in (i).

It is proposed to give, for three special cases, a geometrical construction for the circle based on the proposition that "If PM and PN are the perpendiculars from P on two fixed lines OM, ON then PM/PN is constant for all points on OP"—a proposition of considerable use which should take a place with the standard "locus" propositions.

In each case the simple construction shown in figure (A) is required.

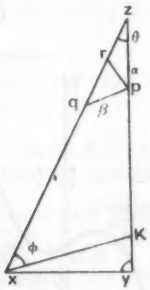


FIG. A.

xyz is a given right-angled Δ and K is determined so that $\frac{zK}{Kx}$ is equal to a given ratio α/β.

This is effected by making $\begin{cases} zp=\alpha, \\ pq=pr=\beta \end{cases}$

and by drawing xK parallel to qp (or pr).

CASE I.— *When the circle touches AB, AC at S, T and cuts off a chord XX' on BC.*

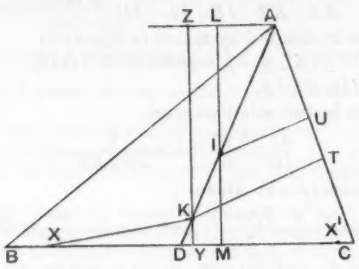


FIG. B.

Suppose K on AD is the centre and I the incentre. Draw a parallel to BC through A and erect corresponding altitudes ZKY, LIM.

Then
$$\frac{ZK}{KX}=\frac{ZK}{KT}=\frac{LI}{IU}=\frac{LI}{IM}=\frac{AI}{ID}=\frac{b+c}{a}.$$

Thus K may be fixed by applying the method of figure (A), with

$$\begin{cases} xy=\frac{1}{2}XX', & yz=YZ=h, \\ a=AI, & \beta=ID, \end{cases}$$

to determine KY.

There will be two solutions if $\dfrac{a}{b+c}>\dfrac{xy}{xz}$, *i.e.* $\dfrac{l}{\sqrt{l^2+h^2}}$.

On reduction this is equivalent to $l<\sqrt{(s-b)(s-c)}$ or $l<\sqrt{rr_1}$.

In general the radii are $l\operatorname{cosec}(\phi\pm\theta)$ and the centres are at distances $l\cot(\phi\pm\theta)$ from BC and on opposite sides of it. θ and ϕ (see Fig. (A)) are given by $h\tan\theta=l$, $a\sin\phi=(b+c)\sin\theta$.

In the special case when $l=\sqrt{rr_1}$ we find that the centre is outside the triangle at distance $\frac{1}{2}(r_1-r)$ from BC. Hence the centre bisects II_1 and the radius is $\frac{1}{2}(r_1+r)$.

CASE II.—*When the circle touches BC and cuts off equal lengths 2l from AB and AC.*

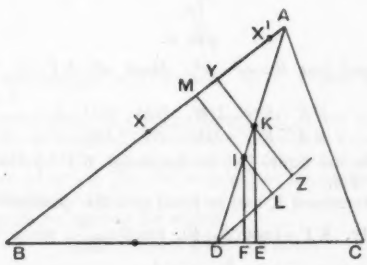


FIG. C.

Draw DZ parallel to BA and let MIL, YKZ be altitudes of the ΔDBA through the incentre I and the required centre K.

Then XX' being one of the chords,

$$\frac{KX}{KZ}=\frac{KE}{KZ}=\frac{IF}{IL}=\frac{IM}{IL}=\frac{AI}{ID} \text{ as before.}$$

Thus the solution is obtained by taking in figure (A)

$xy=l=\frac{1}{2}XX'$, $yz=h'=$altitude of $\triangle DAB$,

$\alpha=ID$, $\beta=IA$.

There will always be two solutions since

$$\frac{AI}{ID}=\frac{b+c}{a}>1>\frac{l}{\sqrt{l^2+h'^2}}.$$

The radii are $l\operatorname{cosec}(\phi\pm\theta)$ where

$$\tan\theta=\frac{l}{h'}=\frac{l(b+c)}{2\triangle} \quad \text{and} \quad \sin\phi=\frac{a}{b+c}\sin\theta.$$

CASE III.—*When the circle cuts off unequal lengths* $2n$, $2m$ *from* AB, AC *and has its centre on* BC.

If K is the required centre and KG, KY perps. to AB, AC, then

$$KG^2-KY^2=(\text{radius})^2-n^2-(\text{radius})^2+m^2=m^2-n^2.$$

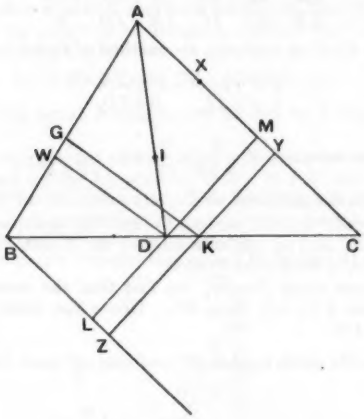


FIG. D.

Take $m^2>n^2$ and put $l^2=m^2-n^2$. Mark off $XY=l$. Then, BZ being parallel to AC,

$$\frac{KX}{KZ}=\frac{KG}{KZ}=\frac{DW}{DL}=\frac{DM}{DL}=\frac{CD}{DB}=\frac{b}{c}.$$

We can therefore use figure (A) to determine KY by taking $\alpha=c$, $\beta=b$, $xy=l$, $yz=LM$ or YZ.

When KY is determined K can be fixed and the $(\text{radius})^2=KY^2+m^2$.

Trigonometrically, $KY=l\cot(\phi\pm\theta)$, $\tan\theta=\dfrac{bl}{2\triangle}$, $\sin\phi=\dfrac{c}{b}\sin\theta$.

The construction is possible if $\dfrac{b}{c}>\dfrac{l}{\sqrt{l^2+YZ^2}}$.

This gives $l^2(c^2-b^2)<4\triangle^2$.

This inequality will always be satisfied if $b\geqslant c$.

If $b < c$ there will be two solutions provided $l < \frac{2\triangle}{\sqrt{c^2 - b^2}}$, but only one in the limiting case $l = \frac{2\triangle}{\sqrt{c^2 - b^2}}$.

In this case the centre is at distances $\frac{2b\triangle}{c^2 - b^2}$, $\frac{2c\triangle}{c^2 - b^2}$ from AC and AB.

NOTE.—(*a*) Case I admits of easy modification to solve the problem when $n = m$ and $l > m$.

(*b*) Case II similarly leads to a solution when $n = m$ but $l < m$.

If $l = m = n$ the solutions are obvious.

(*c*) Case III can be interpreted to cover the case when the centre is on any given line.

The results given above by means of geometrical analysis might all have been deduced from the general equation (i). It would be of interest to know whether the general problem has been solved and, if so, where a solution may be found.

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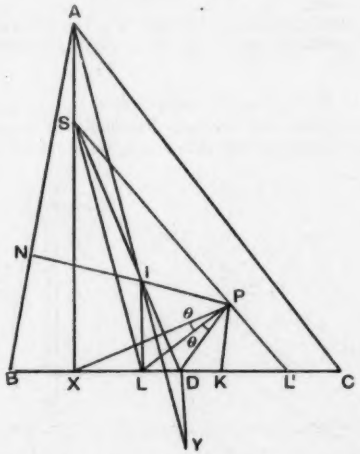
998. [K¹. 2. d.] *A Proof of Feuerbach's Theorem.*



The N.P.C. of the triangle ABC cuts BC at X, D and the incircle touches BC at L, AB at N.

Now if two circles touch at P and a chord XD of one touches the other at L, then PL bisects the angle XPD either internally or externally.

Hence we can find the point of contact of the N.P.C. and the incircle by considering points at which XL, LD subtend equal angles. We are thus led to a proof of Feuerbach's theorem.

All such points lie on a circle for which

$$PX/PD = LX/LD = \lambda \text{ (say)}.$$

Suppose K is the centre, ρ the radius and LL' a diameter of this circle, and let it cut the incircle at P.

Clearly KP is the tangent at P to the incircle and, as $K\hat{P}D = P\hat{X}D$, KP is also the tangent at P to the circle PXD.

Now XD subtends in the major segment of the N.P.C. an angle $(B-C)$. Hence we have to show that $2\theta = B-C$. Now

$$\rho = \tfrac{1}{2}(LD+DL') = KL' \quad \text{and} \quad \lambda\rho = \tfrac{1}{2}(LX+XL') = KX;$$

$$\therefore\ (\lambda+1)\rho = L'X.$$

Further, IK and PL' are perpendicular to PL, so if $L'P$ meets AX in S, SL' and IK are parallel and hence

$$XS/IL = L'X/LK = \lambda+1 = DX/DL;$$

$\therefore$ SID is a straight line.

Next let AI meet the circumcircle in Y.

Then $$AS/AI = YD/YI = YD/YC = NI/AI;$$

$\therefore$ $AS = r$ and $ASLI$ is a parallelogram;

$$\therefore\ \hat{XSL} = \hat{SAI} = \tfrac{1}{2}(B-C).$$

Finally from the cyclic quadrilateral $XSPL$, $\hat{XSL} = \hat{\theta}$.

Notes.—1. $\lambda = (b+c-a)/a$: the figure is drawn with $\lambda > 1$, *i.e.* $b+c > 2a$.

If $b+c < 2a$, L' is on the other side of L.

If $b+c = 2a$, the circle locus reduces to the straight line LI on which the N.P.C. and incircle meet.

2. A similar construction and proof may be given for the excircles.

3. LI produced meets SP on the incircle at J; KI meets SX at T where $TS = AS = r$, etc. B. E. LAWRENCE.

999. [R. 2. b. γ.] *A Centre of Gravity problem.*

A right circular cylinder is bisected diagonally by a plane perpendicular to a plane section containing the axis: it is required to find the C.G. of the half.

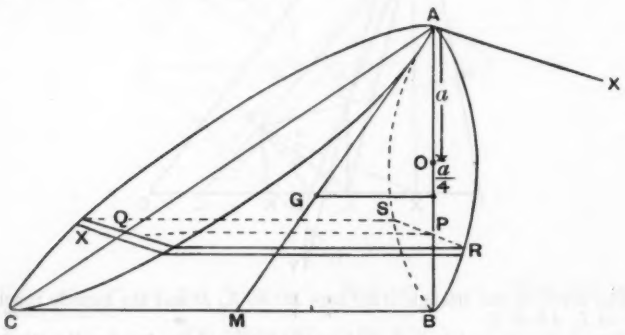


FIG. 1.

By dividing into plates perpendicular to ABC, to the plane of symmetry, and parallel to BC, it is obvious that the C.G. lies on AM, M being the mid-point of BC.

Consider such a plate cutting AB at P.

Let $AP = y$, thickness of plate δy;

$PR = x$, then $PQ = my$ $(m = \tan CAB)$;

mass of plate $= 2xym\,\delta\,y\rho$ $(\rho = \text{density})$

$= bxy\,\delta y.$

If a circular area represented by $ARBS$ is immersed in a liquid with AX, the tangent at A in the surface, pressure at depth y

$$= g\sigma y \ (\sigma = \text{density of liquid})$$

thrust on strip at depth $y = 2g\sigma yx\,\delta y$

$$= bxy\,\delta y$$

where $m\rho = g\sigma$;

$\therefore$ vertical distribution of mass of half cylinder is like the distribution of pressure on circle immersed as above.

Hence depth of C.G. of half-cylinder = depth of C.P. of circle with tangent in surface

$$= \frac{5a}{4}, \text{ i.e. at } G, \frac{a}{4} \text{ from centre of base.}$$

This method may be applied to a portion of a cylinder as in Fig. 2.

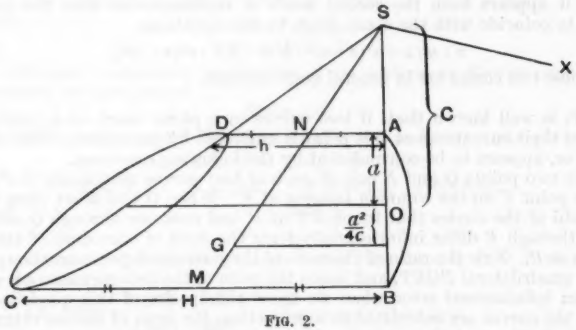


FIG. 2.

The intersection of the planes of the circular and elliptic ends represents the surface of the liquid.

C.G. is on SNM and at depth of C.P. of circle diameter AB, if k is radius of gyration of circle about SX, $\bar{y}$ is depth of centroid of circle,

$$\frac{k^2}{\bar{y}} = \frac{\frac{1}{4}a^2 + c^2}{c} = \frac{a^2}{4c} + c;$$

$\therefore$ G is at depth $\dfrac{a^2}{4c}$ below O.

$$\frac{h}{c-a} = \frac{H}{c+a}; \quad \therefore \ c = \frac{a(H+h)}{H-h};$$

$$\therefore \ \frac{a^2}{4c} = \frac{a(H-h)}{4(H+h)}.$$

A. HINCKLEY.

1000. [L^1; O^1. 2. e; L^2. 17. a.] *Some Geometrical Fallacies.*

1. If we choose according to any law a single point Q on the tangent to a conic C at each point P, the point Q will describe a certain curve as P describes the conic C, so that a point chosen at random in the plane will not in general be a possible position of the point Q.

If we apply this remark to the case where the conic C is imaginary and where we take as the point Q the unique real point that lies on the imaginary

tangent to C at P, we are led to the obviously false conclusion that no tangent can be drawn to C through a real point in the plane unless this point lies on a certain curve.

2. If x, y, z are the homogeneous coordinates of a point in the plane, the equations

$$x : y : z = (t-ka)^2(t-a)^2 : (t-k\beta)^2(t-b)^2 : (t-k\gamma)^2(t-c)^2,$$

in which t is a variable parameter, represent the same quartic curve as the equations

$$x : y : z = (s-a)^2(ks-a)^2 : (s-\beta)^2(ks-b)^2 : (s-\gamma)^2(ks-c)^2,$$

in which s is a variable parameter, for the second set may be derived from the first by writing $t = ks$. If we allow the number k to tend to zero, it appears from the first mode of representation that the quartic tends to coincide with the conic given by the equations

$$x : y : z = (t-a)^2 : (t-b)^2 : (t-c)^2,$$

while it appears from the second mode of representation that the quartic tends to coincide with the conic given by the equations

$$x : y : z = a^2(s-a)^2 : b^2(s-\beta)^2 : c^2(s-\gamma)^2,$$

and these two conics are in general quite distinct.

3. It is well known that, if two curves in a plane touch at a point, the ratio of their curvatures at this point is unaltered by projection. This result, however, appears to be contradicted by the following reasoning.

Take two points Q and R one on each of two curves that touch at P, and take a point T on the common tangent at P. When Q and R are close to P, the radii of the circles that touch PT at P and pass one through Q and the other through R differ infinitesimally from the radii of curvature of the two curves at P. Now the ratio of the radii of these circles depends on the species of the quadrilateral $PQRT$, and hence the ratio of the two curvatures is given with an infinitesimal error when we know the species of this quadrilateral. When the curves are submitted to a projection, the ratio of the curvatures of their projections at the corresponding point P' is similarly determined by the species of the quadrilateral $P'Q'R'T'$ into which $PQRT$ projects. Since it is possible to project a given quadrilateral into a quadrilateral of assigned species, it appears that the ratio of the curvatures at P' of the projections of the given curves may have any value that we please.

4. The two equations

$$S \equiv ax^{\frac{1}{2}} + by^{\frac{1}{2}} + cz^{\frac{1}{2}} = 0, \quad S' \equiv a'x^{\frac{1}{2}} + b'y^{\frac{1}{2}} + d'w^{\frac{1}{2}} = 0$$

in homogeneous coordinates x, y, z, w represent two quadric cones, and the equation $\lambda S - \mu S' = 0$ represents a surface which passes through the common curve C of these cones, whatever values λ and μ may have. In particular, the surfaces represented by the equations

$$a'S - aS' \equiv (a'b - ab')y^{\frac{1}{2}} + a'cz^{\frac{1}{2}} - ad'w^{\frac{1}{2}} = 0,$$

$$b'S - bS' \equiv (ab' - a'b)x^{\frac{1}{2}} + b'cz^{\frac{1}{2}} - bd'w^{\frac{1}{2}} = 0$$

are quadric cones which pass through the curve C. We have thus four quadric cones having a common curve of intersection, and therefore the tetrahedron formed by their vertices, which is the tetrahedron of reference, is self-conjugate with respect to each of these cones. This, however, is inconsistent with the fact that each of the cones touches the three faces of this tetrahedron that pass through its vertex.

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1001. [0^1. 5.] *Simple Vector Methods for some formulae in the differential geometry of surfaces.*

In two recent notes in the *Gazette*, Mr. L. E. Prior* and Prof. E. H. Smart† obtain some results in the differential geometry of surfaces by employing a method involving the multiplication of determinants. This method is also used, for example, by Blaschke‡ in obtaining Gauss's formulae for the second derivatives x_{11}, x_{12}, etc.

All these results can be obtained very easily by the methods given below.

We use the following notation:

$\mathbf{r}$ is the position vector of a point on a surface; u, v are curvilinear coordinates on the surface; $\mathbf{n}$ is the unit surface-normal; suffixes 1 and 2 denote partial differentiation with respect to u and v respectively; scalar products are denoted by round brackets and vector products by square brackets.

$$E \equiv \mathbf{r}_1^2, \quad F \equiv (\mathbf{r}_1\mathbf{r}_2), \quad G \equiv \mathbf{r}_2^2;$$
$$\bar{E} \equiv \mathbf{n}_1^2, \quad \bar{F} \equiv (\mathbf{n}_1\mathbf{n}_2), \quad \bar{G} \equiv \mathbf{n}_2^2;$$
$$L \equiv -(\mathbf{n}_1\mathbf{r}_1), \quad M \equiv -(\mathbf{n}_1\mathbf{r}_2) \equiv -(\mathbf{n}_2\mathbf{r}_1), \quad N \equiv -(\mathbf{n}_2\mathbf{r}_2).$$

Then $$\mathbf{n} = [\mathbf{r}_1\mathbf{r}_2]/V,$$

where $$V^2 \equiv EG - F^2.$$

A rectilinear congruence having the given surface as director surface is represented by an equation

$$\mathbf{R} = \mathbf{r} + t\mathbf{d},$$

where $\mathbf{d}$ is the unit vector in the direction of a ray, and t is a scalar parameter.

$$e \equiv \mathbf{d}_1^2, \quad f \equiv (\mathbf{d}_1\mathbf{d}_2), \quad g \equiv \mathbf{d}_2^2;$$
$$a \equiv (\mathbf{r}_1\mathbf{d}_1), \quad b \equiv (\mathbf{r}_2\mathbf{d}_1), \quad \beta \equiv (\mathbf{r}_1\mathbf{d}_2), \quad c \equiv (\mathbf{r}_2\mathbf{d}_2).$$

Then $$\mathbf{d} = [\mathbf{d}_1\mathbf{d}_2]/v,$$

where $$v^2 \equiv eg - f^2.$$

We make use of the following simple vector identities, **A**, **B**, **C**, **D** being any arbitrary vectors:

(i) $$([\mathbf{AB}][\mathbf{CD}]) = \begin{vmatrix} (\mathbf{AC}) & (\mathbf{BC}) \\ (\mathbf{AD}) & (\mathbf{BD}) \end{vmatrix}; \quad \text{(Lagrange's identity)}$$

(ii) $$[\mathbf{A}[\mathbf{BC}]] = (\mathbf{AC})\,\mathbf{B} - (\mathbf{AB})\,\mathbf{C}.$$

1. We shall first deal with Mr. Prior's note.

(i) The determinant which is there denoted by $\triangle$ is the triple scalar product $(\mathbf{n}[\mathbf{n}_1\mathbf{n}_2])$. To evaluate this both Forsyth and Weatherburn use Weingarten's equations, which express $\mathbf{n}_1$ and $\mathbf{n}_2$ in terms of $\mathbf{r}_1$ and $\mathbf{r}_2$.

We have immediately, however,

$$(\mathbf{n}[\mathbf{n}_1\mathbf{n}_2]) = \frac{1}{V}([\mathbf{r}_1\mathbf{r}_2][\mathbf{n}_1\mathbf{n}_2]) = \frac{1}{V}\begin{vmatrix} (\mathbf{r}_1\mathbf{n}_1) & (\mathbf{r}_2\mathbf{n}_1) \\ (\mathbf{r}_1\mathbf{n}_2) & (\mathbf{r}_2\mathbf{n}_2) \end{vmatrix} = (LN - M^2)/V.$$

It is this method of writing a vector as the vector product of two other vectors and then applying Lagrange's identity which proves so useful in much of this work. It is equivalent to reducing a third order determinant to one of the second order.

(ii) The torsion of an asymptotic line, or of a geodesic, is given by

$$\tau = (\mathbf{n}[\mathbf{n}'\mathbf{r}']),$$

where dashes denote differentiation with respect to the arc-length of the curve. Hence

$$\tau = -\frac{1}{V}([\mathbf{r}_1\mathbf{r}_2][\mathbf{r}'\mathbf{n}']) = -\frac{1}{V}\begin{vmatrix} (\mathbf{r}_1\mathbf{r}') & (\mathbf{r}_2\mathbf{r}') \\ (\mathbf{r}_1\mathbf{n}') & (\mathbf{r}_2\mathbf{n}') \end{vmatrix},$$

and since $$\mathbf{r}' = \mathbf{r}_1u' + \mathbf{r}_2v', \quad \mathbf{n}' = \mathbf{n}_1u' + \mathbf{n}_2v',$$

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* *Math. Gazette*, xiv (1929), p. 458, note 935.

† *Math. Gazette*, xv (1930), p. 264, note 982.

‡ *Vorlesungen über Differentialgeometrie* (1924), vol. i. pp. 78-79.

we have immediately

$$\tau = \frac{1}{V}\begin{vmatrix} Eu'+Fv' & Fu'+Gv' \\ Lu'+Mv' & Mu'+Nv' \end{vmatrix}.$$

(iii) The mutual moment of two consecutive rays of a congruence is Dds^2 where

$$D = (\mathbf{d}[\mathbf{r}'\mathbf{d}']) = \frac{1}{v}([\mathbf{d}_1\mathbf{d}_2][\mathbf{r}'\mathbf{d}']) = \frac{1}{v}\begin{vmatrix} (\mathbf{d}_1\mathbf{r}') & (\mathbf{d}_2\mathbf{r}') \\ (\mathbf{d}_1\mathbf{d}) & (\mathbf{d}_2\mathbf{d}) \end{vmatrix}.$$

Since $$\mathbf{d}' = \mathbf{d}_1 u' + \mathbf{d}_2 v',$$

we have immediately

$$D = \frac{1}{v}\begin{vmatrix} au'+bv' & \beta u'+cv' \\ eu'+fv' & fu'+gv' \end{vmatrix}.$$

2. Professor Smart's problem is as follows:

"A congruence of rays is incident on a reflecting surface. Prove that the following expressions are invariant in the incident and reflected congruences, viz.

$$\frac{1}{v^2}(e\beta^2 - 2f\beta a + ga^2), \quad \frac{1}{v^2}\{ec\beta - f(b\beta + ca) + gab\}, \quad \frac{1}{v}(ac - b\beta)."$$

Let the incident ray $\mathbf{d}$ be reflected at the point $\mathbf{r}$ along the direction $\mathbf{d}'$ ($\mathbf{d}$, $\mathbf{d}'$ being unit vectors). Then

$$\mathbf{d} = \lambda\mathbf{n} - \mathbf{d}',$$

where λ is a scalar. Hence

$$(\mathbf{r}_1\mathbf{d}) = -(\mathbf{r}_1\mathbf{d}'), \quad (\mathbf{r}_2\mathbf{d}) = -(\mathbf{r}_2\mathbf{d}').$$

Now $$[\mathbf{r}_1\mathbf{d}] = \frac{1}{v}[\mathbf{r}_1[\mathbf{d}_1\mathbf{d}_2]] = \frac{1}{v}\{(\mathbf{r}_1\mathbf{d}_2)\mathbf{d}_1 - (\mathbf{r}_1\mathbf{d}_1)\mathbf{d}_2\} = \frac{1}{v}(\beta\mathbf{d}_1 - a\mathbf{d}_2).$$

Similarly $$[\mathbf{r}_2\mathbf{d}] = \frac{1}{v}(c\mathbf{d}_1 - b\mathbf{d}_2).$$

Hence $$\frac{1}{v^2}(e\beta^2 - 2f\beta a + ga^2) \equiv \frac{1}{v^2}(\beta\mathbf{d}_1 - a\mathbf{d}_2)^2 = [\mathbf{r}_1\mathbf{d}]^2 = E - (\mathbf{r}_1\mathbf{d})^2,$$

while $$\frac{1}{v^2}\{ec\beta - f(b\beta + ca) + gab\} \equiv \frac{1}{v^2}((\beta\mathbf{d}_1 - a\mathbf{d}_2)(c\mathbf{d}_1 - b\mathbf{d}_2))$$
$$= ([\mathbf{r}_1\mathbf{d}][\mathbf{r}_2\mathbf{d}]) = F - (\mathbf{r}_1\mathbf{d})(\mathbf{r}_2\mathbf{d}).$$

Both these expressions are clearly unaltered by reflection. Further,

$$\frac{1}{v}(ac - b\beta) \equiv \frac{1}{v}\{(\mathbf{r}_1\mathbf{d}_1)(\mathbf{r}_2\mathbf{d}_2) - (\mathbf{r}_1\mathbf{d}_2)(\mathbf{r}_2\mathbf{d}_1)\}$$
$$= \frac{1}{v}([\mathbf{r}_1\mathbf{r}_2][\mathbf{d}_1\mathbf{d}_2]) = V(\mathbf{nd}) = V\cos\theta,$$

where θ is the angle between the incident ray and the surface-normal. This expression is therefore unaltered by reflection.

3. As examples of other useful formulae which can be obtained most easily by these methods, we add the following [cf. Weatherburn, *Differential Geometry* (1927), pp. 61, 64, 65]:

(i) Since $\mathbf{n}$ is a unit vector, $\mathbf{n}_1$ and $\mathbf{n}_2$ are both perpendicular to $\mathbf{n}$. Hence

$$[\mathbf{n}_1\mathbf{n}_2] = \lambda\mathbf{n},$$

where λ is a scalar. To find λ, we have

$$\lambda = (\mathbf{n}[\mathbf{n}_1\mathbf{n}_2]) = \frac{1}{V}([\mathbf{r}_1\mathbf{r}_2][\mathbf{n}_1\mathbf{n}_2]) = (LN - M^2)/V.$$

Thus $$[\mathbf{n}_1\mathbf{n}_2] = \frac{T^2}{V}\mathbf{n},$$

where $$T^2 \equiv LN - M^2.$$

Hence

(ii) $[\mathbf{n}_1\mathbf{n}] = \frac{V}{T^2}[\mathbf{n}_1[\mathbf{n}_1\mathbf{n}_2]] = \frac{V}{T^2}\{(\mathbf{n}_1\mathbf{n}_2)\mathbf{n}_1 - \mathbf{n}_1^2.\mathbf{n}_2\} = \frac{V}{T^2}(\bar{F}\mathbf{n}_1 - \bar{E}\mathbf{n}_2).$

Similarly $[\mathbf{n}_2\mathbf{n}] = \frac{V}{T^2}(\bar{G}\mathbf{n}_1 - \bar{F}\mathbf{n}_2).$

(iii) Also $[\mathbf{n}_1\mathbf{n}] = \frac{1}{V}[\mathbf{n}_1[\mathbf{r}_1\mathbf{r}_2]] = \frac{1}{V}\{(\mathbf{n}_1\mathbf{r}_2)\mathbf{r}_1 - (\mathbf{n}_1\mathbf{r}_1)\mathbf{r}_2\} = \frac{1}{V}(L\mathbf{r}_2 - M\mathbf{r}_1).$

Similarly $[\mathbf{n}_2\mathbf{n}] = \frac{1}{V}(M\mathbf{r}_2 - N\mathbf{r}_1).$

(iv) $(\mathbf{n}[\mathbf{n}_1\mathbf{r}_1]) = \frac{1}{V}([\mathbf{r}_1\mathbf{r}_2][\mathbf{n}_1\mathbf{r}_1]) = \frac{1}{V}\begin{vmatrix} (\mathbf{r}_1\mathbf{n}_1) & (\mathbf{r}_2\mathbf{n}_1) \\ \mathbf{r}_1^2 & (\mathbf{r}_2\mathbf{r}_1) \end{vmatrix} = (EM - FL)/V.$

Similarly
$$(\mathbf{n}[\mathbf{n}_1\mathbf{r}_2]) = (FM - GL)/V,$$
$$(\mathbf{n}[\mathbf{n}_2\mathbf{r}_1]) = (EN - FM)/V,$$
$$(\mathbf{n}[\mathbf{n}_2\mathbf{r}_2]) = (FN - GM)/V.$$

University College, Cardiff. H. A. HAYDEN.

1002. [X. 2.] *The Theorem of Proportional Parts.*

The following method of obtaining this theorem seems to be simpler than that usually given.

Let $$\phi(x) \equiv \begin{vmatrix} f(x) & x^2 & x & 1 \\ f(a) & a^2 & a & 1 \\ f(b) & b^2 & b & 1 \\ f(c) & c^2 & c & 1 \end{vmatrix}$$

be a function of x which possesses a second derivative at all points of the interval (a, c), where $a < b < c$.

Then clearly $\phi(a) = \phi(b) = 0$, hence by Rolle's Theorem, $\phi'(x)$ vanishes at some point ξ between a and b. Similarly, $\phi'(x)$ vanishes at some point η between b and c.

Thus $$\phi'(x) \equiv \begin{vmatrix} f'(x) & 2x & 1 & 0 \\ f(a) & a^2 & a & 1 \\ f(b) & b^2 & b & 1 \\ f(c) & c^2 & c & 1 \end{vmatrix}$$

vanishes when $x = \xi$ and when $x = \eta$, hence $\phi''(x)$ vanishes at some point ζ between ξ and η.

But $$\phi''(x) \equiv \begin{vmatrix} f''(x) & 2 & 0 & 0 \\ f(a) & a^2 & a & 1 \\ f(b) & b^2 & b & 1 \\ f(c) & c^2 & c & 1 \end{vmatrix},$$

which, on expansion, becomes

$$\begin{vmatrix} a^2 & a & 1 \\ b^2 & b & 1 \\ c^2 & c & 1 \end{vmatrix} f''(x) - 2 \begin{vmatrix} f(a) & a & 1 \\ f(b) & b & 1 \\ f(c) & c & 1 \end{vmatrix},$$

i.e. $$-(b-c)(c-a)(a-b)f''(x) - 2\Sigma(b-c)f(a).$$

Hence a number ζ exists, between a and c, such that

$$\frac{f(a)}{(a-b)(a-c)} + \frac{f(b)}{(b-c)(b-a)} + \frac{f(c)}{(c-a)(c-b)} = \tfrac{1}{2}f''(\zeta);$$

whence $$f(b) - f(a) = \frac{b-a}{c-a}\{f(c) - f(a)\} + \tfrac{1}{2}(b-a)(b-c)f''(\zeta),$$

which is the theorem of Proportional Parts. N. R. C. DOCKERAY.

1003. [C. 1. f.] *The criteria for maxima and minima.*

The complete tale of conditions for maxima and minima is always given as a corollary to Taylor's theorem. Actually, however, this tale is a corollary to the simple mean-value theorem which asserts the existence of a positive proper fraction θ_1 such that $f(a+h)=f(a)+hf'(a+\theta_1 h)$.

With $f'(x)$ substituted for $f(x)$ and $\theta_1 h$ for h, the same theorem asserts the existence of a positive proper fraction θ_2 such that

$$f'(a+\theta_1 h)=f'(a)+\theta_1 hf''(a+\theta_1\theta_2 h);$$

with $f''(x)$ substituted for $f(x)$ and $\theta_1\theta_2 h$ for h, the theorem asserts the existence of a positive proper fraction θ_3 such that

$$f''(a+\theta_1\theta_2 h)=f''(a)+\theta_1\theta_2 hf'''(a+\theta_1\theta_2\theta_3 h);$$

continuing the process, and ultimately substituting in the first formula, we establish the existence of n positive proper fractions $\theta_1, \theta_2, \dots \theta_n$ such that

$$\begin{aligned} f(a+h)=f(a)+hf^{(1)}(a)+\theta_1 h^2 f^{(2)}(a)+\theta_1^2\theta_2 h^3 f^{(3)}(a)+\dots \\ +\theta_1^{n-2}\theta_2^{n-1}\dots\theta_{n-2}h^{n-1}f^{(n-1)}(a) \\ +\theta_1^{n-1}\theta_2^{n-2}\dots\theta_{n-1}h^n f^{(n)}(a+\theta_1\theta_2\dots\theta_n h). \end{aligned}$$

Hence, if the derivatives $f^{(1)}(a)$, $f^{(2)}(a)$, $\dots f^{(n-1)}(a)$ all vanish, there are numbers θ, ϕ such that

$$f(a+h)-f(a)=\theta h^n f^{(n)}(a+\phi h),$$

and these numbers, each being a product of positive proper fractions, are themselves positive proper fractions. For the discrimination of maxima and minima, only the sign of $f(a+h)-f(a)$ is wanted, and the multitude of the unknown fractions $\theta_1, \theta_2, \dots \theta_n$ is irrelevant; all that it is necessary to know is that θ is positive and that ϕ is a proper fraction, for these conditions imply that if $f^{(n)}(a)$ is not zero, then $f(a+h)-f(a)$ has the sign of $h^n f^{(n)}(a)$ for sufficiently small values of h. E. H. N.

1004. [M⁴. b. α.] *The Curve of Pursuit.**

In the Appendix to the *Gentleman's Diary* for 1839 appeared an article "On the Curve of Pursuit" by Samuel Jones, which concluded (p. 101) as follows:

The question from which *the Curve of Pursuit* had its origin was proposed about the middle of the last century, in one of the periodicals of the day. It related to the chase of a spider and fly on a rectangular pane of glass; and from the artifice employed in the solution (see *Simpson's Fluxions*, prob. 15), elicited considerable attention from the periodical correspondents at that time. *See* Simpson's original solution; Emerson's ditto to prize question in the *Ladies' Diary* for 1743; Landen's ditto to prize question in ditto for 1750; since then it has been solved by Mr. Cunliffe, question 343, *Leybourn's Repository*. But most of these solutions suppose the directions of the bodies, at the commencement of the chase, to be at right angles to each other, and all of them relate only to that part of the curve which is convex to its axis. It may be further remarked, that neither Dr. Hutton, nor any of the commentators on the solutions and questions in Leybourn's republication of the Diaries have noticed that the solutions hitherto given fail when the velocities of the bodies are equal. From this consideration it occurred to me that the above general investigation of the problem would not be unacceptable to readers of the Diary.

[What is the continental history of the problem ?] E. H. N.

* See Problem Bureau, *Gazette*, vol. 15, p. 160.

REVIEWS.

Projective Geometry. By J. W. YOUNG. Pp. ix + 185. $2. 1930. The Carus Mathematical Monographs. No. 4. (Open Court.)

I suppose most people make three distinct approaches to projective geometry. In the first approach the elementary geometry is used and generalised; for instance, the harmonic property of the quadrilateral is shown by projecting the third diagonal to infinity and using a theorem on the parallelogram; or Desargues' theorem on involution in connection with four-point conics is shown by projecting two of the base points to the circular points at infinity and using a theorem on the circle proved in elementary fashion. In the second approach everything depends on cross-ratio, but this notion is defined by means of distance and its fundamental properties are shown metrically. When these are established, the definition is no longer needed. Elementary theorems used in the first method are now seen to be special cases of projective theorems now proved without the aid of the former. In the third approach, projective geometry is built up without using any metric notions whatever; an axiomatic basis is laid down, and everything moves inside the projective domain. Metric notions themselves are defined in terms of projective ideas. This is the standpoint of Baker's *Principles*, and Veblen and Young's *Projective Geometry*.

The present monograph is by one of the authors of the work last mentioned; it is introductory in nature and as far as possible uses the third method. Recognising however that this method, if used with full severity, would be too difficult for beginners, the author uses intuition when he thinks it advisable. Thus to establish the fundamental theorem, he uses the intuitive notions of continuity and of the order of points on a line.

The book begins with an account of the main projective assumptions and figures, leading on to the principle of duality, the theorem of Desargues on perspective triangles, harmonic sets and involutions, treated non-metrically. Then follows a most interesting chapter on the projective geometry of a conic, again non-metric. The connection between projective and metric properties is then considered, and this leads on to a treatment of foci and to the usual metric definitions of conics.

One of the most important discoveries last century in Geometry was contained in v. Staudt's proof that coordinates could be introduced in pure projective geometry without using any idea of distance. The author gives a proof of this in the last chapters; starting with the consideration of groups of projective transformations, he introduces coordinates by the elegant methods of Veblen and Young's book, and thence proceeds to the algebraic representation of projective transformations. Finally the position of Euclidean geometry inside the projective scheme is very clearly treated.

As an introduction to the third method of approach this book could hardly be better. Strict logic and intuition are most skilfully combined, and anyone who has mastered its pages would be in a position to appreciate the more severe treatises on the subject. H. G. F.

Moderne Algebra (erster Teil). By B. L. VAN DER WAERDEN. Pp. viii + 243. Rm. 15.60 (geb. 17.20). 1930. Grundlehren der Mathematischen Wissenschaften in Einzeldarstellungen, xxxiii. (Springer, Berlin.)

By the author's own confession, the book is *not* an "introduction to modern algebra" but a work which presupposes, on the part of its readers, a previous knowledge of most things in algebra which may be termed "classical". For those select few members of the Mathematical Association who are already knowledgeable concerning Corps, Ideals, Groups and the vagaries of curious algebras, here is the complete logical presentation they have desired. It is indeed possible to use the book as a first, post-graduate reading of modern developments, but, when so used, it would be satisfying only to the extremely devoted lover of the abstract and theoretical.

The second volume will be concerned with special branches of algebra, while this first volume provides the foundation of the work in the theory of groups and "körper". The whole book stresses the abstract and axiomatic line of argument. The first three chapters form an attempt to give a short but

complete groundwork for elementary algebra and the theory of groups. The author recognises the fact that recent writings on special aspects of his subject have provided the student with any *detail* he may require, and so is reinforced in his own intention to concentrate on the logical development of the main theory. The effort to make this groundwork "*knapp aber lückenlos*" (to use the author's own words) has resulted in one or two illustrations of the well-known fact that such a task is beset with pitfalls. Thus the name "symmetric group" is, at the moment when it first occurs, a purely arbitrary piece of nomenclature which might equally well be replaced by "the sky-blue group" were it not for the fact that the reader anticipates the author's later explanation of what is meant by a symmetric function. Again, and this time it is "lückenlos" and not "knapp" that is responsible for my amusement, one is solemnly informed that the meaning of the symbol

$$\prod_{1}^{n} a_\nu$$

is unaffected if another letter replace ν!

Let it be said that the book is well and carefully written and that its explanations are clear. It cannot be said that this book will do much for algebra save among those mathematicians who are already interested in and have studied the abstract side of it. What is wanted for this country, and wanted with some degree of urgency, is a book which will develop algebra as a theory *and technique* which become stronger and more formidable as the book progresses. Let us see the edifice go up and realise how the parts are put in place. I have heard that an English introduction to modern algebra is in preparation: it will be a welcome addition to our text-books. But I hope the authors will be wary of that "lückenlos"—I should hate to be told, in English, that I might write either n or m as a typical suffix. W. L. F.

A Short History of Mathematics. By VERA SANFORD. Pp. xii + 402. 10s. 6d. 1930. (Houghton Mifflin Company: Harrap.)

Of the two methods used in writing histories of mathematics, the chronological has the advantage that the order of development of the subject is clear, and that the opportunities—and difficulties—which confronted mathematicians are brought into close relationship with the cultural tendencies of the period. The alternative method which, after a rapid general survey, deals with special topics tends to obscure these relationships, and consequently removes or greatly lessens one main objective of the student. The interest aroused by the second method is perhaps more lively, but the general impression which remains will be less accurate.

The book under review follows the latter method and may be compared with the *History of Mathematics* of Professor D. E. Smith, to whom Miss Sanford acknowledges her debt. The smaller bulk (and price) will probably appeal to many who wish to know how certain branches of mathematics have developed, and the book can be recommended as an accurate and interesting survey within its limitations. It deals only with *elementary* mathematics, a term which excludes whatever is not included in the school course. The chronological introduction is slight, and, one hopes, will whet the appetite for other, more comprehensive, works. The author's main interest obviously lies in the less mathematical topics which occupy many of the other chapters. Her treatment of the abacus, verbal problems, commercial mathematics, and others is excellent, but some of her statements in the chapters on geometry and algebra are open to question. The destructive force of Zeno's paradoxes is not appreciated, as they were equally fatal to the atomicist and the believer in infinitesimals (to whom the author confines their application). It was these, and not the three problems, which led to the invention of the method of exhaustions, and it is surely far-fetched to connect the latter method with the work of Antiphon. One would have liked to see the solution of the duplication problem due to Archytas given in full. It is one of the most beautiful pieces of Greek geometry. It is incorrect to state that Euclid's *Elements* closed the way to further development! He was followed by Archimedes and the "Great Geometer", Apollonius.

Misprints are small in number. It should be noted that several errors occur in the statement of Vieta's solution of the quadratic (p. 168), z should be replaced by 2 in the equation of the catenary (p. 190), Constantinople fell in 1453 (p. 296), *abscissa* is probably due to C. Wolf (p. 306), and 106 should be 105 (p. 342).

Teachers will find the book of considerable use in providing interesting subjects to enliven the more formal parts of their teaching, and whole-hearted commendation can be given to the wealth of illustration which is one of the most admirable features of a useful and attractive contribution to the history of mathematics. C. W. GILHAM.

A School Geometry. By H. G. FORDER. Pp. ix + 259. 4s. 6d. 1930. (Camb. Univ. Press.)

School Certificate Geometry. By F. F. POTTER and DENHAM LARRETT. Pp. vi + 200. 3s. 6d. 1930. (Isaac Pitman.)

Senior Geometry. By A. E. TWEEDY. Part 1, pp. ix + 236. Part 2, pp. ix + 180. 2s. 3d. each. 1930. (Dent.)

Thirty years and more ago, before the revolution which replaced "Euclid" as a school subject by "Geometry", the young boy began by learning by heart a set of definitions, postulates and axioms. There followed propositions, arranged in sequence so that each was deduced from the members preceding it, all resting on the groundwork of definitions, postulates and axioms. Euclid was an ideal science. Any figures that one drew on the blackboard or on one's paper were only symbols of reality. The lines were all too thick. Besides, Euclid had curious old-fashioned compasses which shut up directly he removed them from the paper (or from his sanded floor). There was a lot of involved language, seemingly a deal of fuss over rather little, and some of it—the definition of proportion for instance—was as obscure and incomprehensible as the Athanasian Creed. There was also a subject called Geometrical Drawing studied in army classes and by people who were going to be architects or engineers. This was merely utilitarian—a sort of poor relation of Euclid's.

In these days the study of Geometry starts with Geometrical Drawing. The beginner uses instruments to draw figures from which he makes measurements. From these certain geometrical facts are induced. Then the process of deduction begins, and propositions follow much in the style of Euclid, except that the language is clarified and certain difficulties are, as a rule, carefully evaded. Geometry has thus become a "real" subject (dealing, that is, with the actual lines and circles drawn on the paper—the fact that theorems established by deduction refer to idealisations of these is generally lost sight of), and, as a school subject, interesting and easy—easier at any rate than Algebra.

But has the change been all for the good? The trouble is that, in these examination-ridden days, when every boy and girl must "pass in Maths.", we tend to peptonise the stuff we give them, so as to make it palatable to the weaker digestions. But what is happening to pupils of natural mathematical ability? Is there not real loss in their case?

In the Euclid days there was much learning by rote, much unintelligent repetition of verbal formulae, but at any rate the boy of natural ability got to realise what a valid deductive proof meant. As he grew up, the obscure phrases in the definitions gradually acquired meaning. If he was exceptionally bright, he began to have doubts as to the strictness of Euclid's logic, and to ask awkward questions about the congruency proofs and the twelfth axiom. It is probably not an exaggeration to say that, nowadays, very few pupils aged, say, fifteen, realise clearly the full nature of a deductive proof. Geometry is so well taught up to a point that most boys and girls can do, with tolerable ease, the simple two- or three-step rider which abound in the books and in examination papers, in which a theorem is given as a starting point, and the road to the desired conclusion is short and well marked out. There is value, educationally, in reaching this stage. But what happens when the road is rather longer and not so straight, when there needs to be a preliminary construction, or a suitable selection has to be made from a number of possible

jumping-off places ? The writer's experience, which he knows to be shared by others, is that too often what happens is as follows. An intelligent pupil will produce half a dozen lines of correct, if irrelevant, deduction, followed by an unblushing *petitio principii* and the letters Q.E.D. If one confronts such a pupil with his proof and enquires, " Why do you say here that $OA = OB$? " the answer is almost invariably one of two, either " I thought they must be equal " (meaning that the desired conclusion would follow if they were) or " I measured them from the figure and found them equal and so thought they must be ". The trouble is that there is no bridge to unite but yet separate the terrain of the empirical, inductive methods of the early stages of geometrical teaching and that of the stricter, deductive methods of the later stages. So ideas from the first stage do not, without difficulty, get extricated from those of the second. This, in the writer's opinion, is the real trouble, and the recent demand for an authoritative sequence is an attempt to deal with a symptom, not the disease.

Three years ago Mr. H. G. Forder published a remarkable and erudite book on *The Foundations of Euclidean Geometry*, in which he analysed the fundamental axioms underlying Euclidean (and certain non-Euclidean) geometries. Both in the preface of this book and in the text he expressed himself somewhat strongly on the subject of the logical incoherence of most School Geometry texts. Many readers of his book must have felt that there was a great deal in his point of view, but have wondered if it were possible to put the new geometrical wine into Euclidean bottles. In other words, is such a bridge as that suggested above feasible ? Mr. Forder has now produced his *School Geometry* ; it is a book which demands careful consideration from all who wish to see further improvement in the teaching of the subject.

His broad idea is as follows. By drawing and measurement certain facts are suggested (as, for instance, that vertically opposite angles are equal). These are assumed to be true. Nine of these assumptions are thus justified and carefully stated. Then the process of deduction begins, and a sequence of theorems and constructions follows. For a time drawing work is carried on simultaneously, and other assumptions are thus introduced as required and used as additional bases for deduction. The test that this set of sixteen assumptions is " accurate " is that the Geometry deduced from them is consistent. If any theorem could be deduced from them which contradicted another, it would follow that one or other of the assumptions was unjustified. (The resulting Geometry is based on experience and so is presumably the Geometry of space. That this may not be so is irrelevant.) The whole of the theorems and constructions embraced under " School Geometry " are thus included, either as assumptions or among the deduced propositions. As the process of drawing and induction fades out, the standard of rigour in the matter of deduction is gradually raised. Finally comes a reconsideration of the body of assumptions. A couple of " existence " axioms are introduced to underpin the structure, and it is shown that some of the assumptions can now be proved. Unnecessary props are thus knocked away, and a note is added to the effect that a further process of underpinning and a consequent further reduction in the number of props is possible, but that this belongs to a more advanced course. All this may sound rather alarming, but it is set out in simple and natural language and need cause no difficulty to pupils of average ability.

But this does not exhaust the content of the book. There are many examples, some worked out, others set as exercises. These are usually grouped round some central idea. Many of these are original, others collected from a wide field. From this aspect alone the book is most stimulating.

There are digressions into bits of country lying off the main route, and difficult fences are not evaded. Mr. Forder writes in a lucid and vigorous style ; his note on incommensurable magnitudes and irrational numbers, and his clear exposition of just where proofs by superposition fail, are models of how this sort of thing should be done. The normal correctness of his language throws into stronger relief one striking case of slipshod expression. In his definition of congruent triangles he says, " . . . when the three sides of one triangle have the same length as the three sides of the other . . . ", which

implies merely that the perimeters are equal. Algebraical symbols are freely used, but not trigonometrical ratios. There is a good deal of work on solid figures and dihedral angles, but no formal treatment. The author clearly distinguishes from the start between "line", "interval" and "ray", a reform which, as he says, has been long overdue.

The preface to his earlier book contained the sentence: "It might be thought that when the foundations had been laid, one could refer to the school texts for this portion of the work; and this I should have done but for the fact that scarcely one proof in any school text will survive a critical examination even from the point of view of the writer, still less from ours". The reader, therefore, turns naturally to the proofs in this later book expecting to find them different from those ordinarily given. With surprise, but considerable relief, he finds that in most cases they are precisely the same as in other school texts, though it is true they are made to rest on *explicit* assumptions instead of on assumptions which were either implicit or confused in the minds of the authors. In two or three cases Mr. Forder's proofs contain unusual features. One of the tangency proofs is differently expressed in order to avoid another assumption. The two *reductio ad absurdum* proofs of the converses to the theorems on angles in the same segment and cyclic quadrilaterals include possibilities which are generally overlooked and the omission of which invalidate the proofs. The theorems are all admirably set out with careful reasons for all statements. Mr. Forder is a skilled teacher and knows just where are the danger spots in setting out deductive proofs. He hoists the red flag at the appropriate places.

The difficulty of the author's task was referred to above. While, in the opinion of the writer he has been on the whole successful in attaining his object, it may be permissible to criticise certain details in the order of presentation. He has put into the preface the disarming sentence: "In choice of terms, in degree of exactness of expression, in order of treatment, as in type of demonstration, I have been guided by psychological rather than by logical necessities". It is not always easy to see whether considerations of psychology or logic have been paramount. He wisely avoids defining angle, but in his first sentence the concept of a right angle is given as that of a quarter of a complete revolution. The succeeding set of examples deals with the measurement of angles in degrees. Then on page 4 we find the assumption, "All right angles are equal to one another". Surely, on grounds both of psychology and of logic, this is placed either too early or too late. Euclid in his fourth postulate "asserts that a right-angle is a determinate magnitude in terms of which other angles can be measured".* It implies also "the antecedent postulate of the invariability of figures or its equivalent, the homogeneity of space".* What, however, can it mean placed thus to a child who has already been measuring angles? It is suggested that it would have come better after the first sentence in the book, with one of those simple comments which Mr. Forder knows so well how to write, or that it should have been deferred till later and coupled up with the existence axioms on page 218.

The treatment of locus is not altogether satisfactory. The term is introduced without preparation in an exercise on page 34, explained and treated with envelopes in a set of drawing exercises on pages 40 *et seqq*. The deductive treatment of the two familiar locus theorems is deferred to a stage later than usual. The theorems are carefully set out, showing that every point satisfying the given conditions lies on the locus and that every point on the locus satisfies the given conditions. He adds, however, a curious note on page 110 to a worked example, suggesting that this double aspect is not really necessary. In a book which is in other respects so comprehensive one would have expected an example to emphasise the importance of the double investigation so as to ensure that the locus deduced is complete (*e.g.* C is a fixed point on the perpendicular bisector of the interval AB. What is the locus of P so that angle APC = angle CPB?). The theorem "The perpendicular to a chord of a circle from the centre bisects the chord" appears as a corollary to its converse. It would have been better to have set it out as a separate theorem, as it involves

* Heath, *The Thirteen Books of Euclid's Elements*, Vol. I, p. 200.

that case of congruency which is most often misquoted. (It had occurred previously as an exercise.) The work on Similar Triangles is divided up and separated by the "systematising" section in which the assumptions are considered. This is presumably done in order to avoid introducing further assumptions at this stage, unless it is a subtle way of calling attention to a section which, if placed at the very end of the book, might be generally omitted. It seems a pity thus to divide up what is really one section. One would have expected on page 67 the explicit assumption that congruent triangles are equivalent. There is no formal statement to this effect in the definition on page 14 (although the previous paragraph states that such triangles are "obviously of the same size").

The book is emphatically one which should be read by all who are engaged in teaching school mathematics. It cannot fail to be stimulating and suggestive. Whether, in its present form, it will prove an easy book to handle for class purposes is open to doubt. It is admirably printed and attractive in every way. But, right from the beginning, we find starred exercises, starred paragraphs and sections. These are, presumably, meant to be omitted at a first reading. Many of these are too difficult at any reading for any but really able pupils. Intelligent boys and girls who can work through the book completely cannot fail to reap great advantage. But, at most schools, it is not possible to separate the sheep from the goats at an early stage. There are many teachers to-day who feel that in these days of talkies and wireless, when so much information is gathered aurally, it is a wholesome corrective to keep their pupils down to text-books for reading, not only for exercises and recollection, and to let oral teaching be guidance rather than exposition. For such a purpose a book in which many sections have to be omitted is not ideal. Perhaps, therefore, it would have been better if some of the starred examples and sections had been put in a separate part at the end of the book with references from the text.

School Certificate Geometry, by Messrs. Potter and Larrett, is the antithesis of the last book. The object is to provide a course to prepare pupils to pass certain examinations. In this it will probably be successful. The point of view is indicated by one word in the preface. "We have assumed that the way will have been prepared by a suitable course of practical work . . . and that some of the more elementary results will have been established by such methods." It is a matter of speculation what exact meaning the authors here attach to the word "established". They go on to explain that no mention is made of the fundamentals of the subject and the logical consequences of the assumptions upon which any system of Geometry can be built, as they are doubtful whether such considerations can be usefully included in the study of the subject at this stage. This is the opinion directly opposite to that of Mr. Forder.

The familiar theorems are set out quite clearly with exercises of the usual type. These make dull reading after those of the first book. There are not enough reasons given for statements in setting out proofs. For instance, the particular case of congruency employed is not, as a rule, stated. This omission may land an examiner in a difficulty. Thus in the theorem that the perpendicular from the centre of a circle to a chord bisects that chord, the proof given reads as follows: "In triangles OAM and OMB, $OA = OB$ (radii of circle), OM is common to both triangles, angle $OMA = 90° =$ angle OMB; $\therefore$ triangles are congruent".

What does "$= 90°$" mean here? Does the candidate understand that this is not merely a condition for the equality of the angles, but also a necessary part of the test for the congruency? If he finishes "triangle $OAM \equiv OBM$ (2 sides and right angle)", or something equivalent to this, there is no doubt. Without it many examiners feel that they are not justified in awarding full marks for the proof.

Mr. Tweedy's book lies somewhere between the other two. It is not so original as the first nor so commonplace as the second. The author has a clear conception of the meaning of a deductive proof and of standards of rigour. The book presupposes inductive work on the lines of his *Junior Geometry*. The Basic Facts induced are clearly set out and stress is now laid

on deduction. The work proceeds by a series of exercises in the course of which the standard theorems are all proved. This is essentially a class book and the teacher need do little else but guide. The language used is simple and exact, and quite slow pupils should be able to make steady progress by following the book. At the end of each part the standard theorems are collected and set out in proper form. Part 1 takes us to the end of circle properties and Part 2 to the end of similarity. For able pupils the rate of progress might be too slow. The author anticipates this objection and suggests that for such pupils the two parts may be used concurrently, the second part containing also harder revision examples on the material of the first. At the end of Part 2 there is a short section on " Basic facts " or axioms. This section is disappointing in that he introduces a proof by superposition and a parallel axiom without a word as to the difficulties involved in these assumptions. A couple of additional pages at the end would have rounded off the book.

H. E. PIGGOTT.

Contributions to the History of Determinants, 1900-1920. By SIR THOMAS MUIR. Pp. xxiii + 408, with index to all preceding volumes. 30s. 1930. (Blackie.)

This excellent volume is part of a very remarkable series of works upon the History of Determinants which are well known throughout the mathematical world: and, for many years already, the name of this admirable author has been associated with determinants and all their ramifications. By his early text-book (which has just been revised by Metzler and now runs to 750 pages), and still more by the systematic production of the four volumes which gave well-nigh a complete account of all that had been written on determinants before the present century, the author had performed a work of conspicuous service to mathematicians of all types. The first instalment of the four-volume *History of Determinants* (Macmillan) was published in 1890, the second in 1911, the third in 1920, and the fourth in 1923—a truly formidable programme to be carried out by one whose professional duties were administrative, as leader in the Public Education at the Cape, who lived for forty years 6000 miles away from reference libraries, and who only attained leisure to think consecutively upon mathematics when in 1915 he retired from his official duties.

Forty-nine years ago, the author published his first " List of Writings on Determinants " in the *Quarterly Journal of Mathematics*; he has now produced the present volume, which brings the history almost up to date. The book has been compiled and printed with great care, and in every way is a worthy coping stone to this impressive edifice. The new publishers have brought out a volume slightly smaller but fully in keeping with those that have preceded it. Except for very minor details, each earlier volume was complete in itself; but the present book departs from the tradition by excluding chapters on certain definite branches of the theory, Bigradients, Circulants, Continuants, Hessians, Jacobians, Orthogonants, Persymmetrics, Skew and Pfaffians. These appeared mostly in the *Proceedings of the Edinburgh Royal Society*, with the exception of Hessians and Hadamard's approximation, which may be found in the *Transactions of the Royal Society of South Africa*. Bereft as it is of these varied chapters, the present volume still has plenty to deal with, running to 400 pages on Determinants in General, Text-books, Linear Equations, Axisymmetric Determinants, Alternants, Compound Determinants, Recurrents, and various less special forms. As in the previous volumes, there is a clear statement of each writing on determinants, as it comes under review, with a running commentary which abounds in suggestive sidelights and cross-references. At the risk of redundancy the book (and naturally the whole series of volumes) aims at being complete, every rediscovery of well-known or little known theorems being duly chronicled, maybe up to the tenth repetition. But all this is brought to light, and every effort is made to stress the salient points, and to draw the attention of the reader to what Macmahon would call the Master Theorems of the subject. A notable and welcome addition to the value of this book is the complete index of all five volumes

with which it ends. It is truly marvellous that such a work, stimulating and interesting as ever, and exhibiting no trace of flagging energy, should have been produced by an author who has passed his eighty-fifth year. In the words of Sylvester, who inspired so many early pages of the *History*, "the mathematician lives long and lives young; the wings of his soul do not early drop off, nor do its pores become clogged with the earthy particles blown from the dusty highways of vulgar life". H. W. T.

Mathematische Volkswirthschaftslehre. Eine Einführung von Dr. Otto Weinberger. Pp. 242. Rm.18. 1930. (Teubner.)

Very little attention has been given to the mathematical treatment of political economy in Germany in recent years, except by isolated students. This may be in part due to the absence of any text-book in German on the subject, and Dr. Weinberger has set out to remedy this defect. It may be said at once that this book is not of any great importance to those who have access to existing works in English (including American), French or Italian; for there is no claim to originality of method or results, except that the very important problem of obtaining equations that envisage a dynamic instead of a static condition of society is carried further than in Professor Moore's treatment.

As an account of the progress of the mathematical exposition of economic theory and of the ideas and contributions of writers of many countries, this study has its interest and utility. But a student who is not already well acquainted with the problems will find the method of exposition very confusing. An historical setting can hardly allow a clear didactic. The central problem of mathematical economics, that of equilibrium and departure from and approach to equilibrium, occupies only a quarter of the book, and even there the symbols are not clear and the sections are not well knit together. While this study is left incomplete, the latter part of the book contains a very slight exposition of the elementary mathematics needed and an outline of statistical method.

The author has the necessary knowledge and insight to write a useful book, compiled from the material he has assembled in his Part III. It is to be regretted that he has not so far better appreciated the needs of a student.

A. L. B.

Methods of Correlation Analysis. By Mordecai Ezekiel. Pp. xiv + 427. 22s. 6d. 1930. (New York: Wiley; London: Chapman and Hall.)

The author of this book has refrained, no doubt wisely, from taking all statistics as his province. Instead, he has confined himself to that part of the subject which is concerned with the relations between variables. That a book of the size of this one is the result is a tribute to the author, who has made this the particular field of his own researches, and has done much in the way of determining methods for studying multiple regression and correlation, both planar and curvilinear. The author is Assistant Chief Economist to the U.S. Federal Farm Board, and his illustrative examples are drawn from the domain of agricultural economics, although the principles and methods described are general. The book is exceedingly well written, and for clarity of exposition could scarcely be bettered, so much so that the beginner in statistics, even if his mathematical attainments be of the most limited character, would be well advised to study the opening chapters. Mathematical proofs are not given in the text, but technical notes are added in the appendix in which proofs of the main propositions are given. A most valuable feature of the book is the way in which the results of the most modern researches are adopted throughout in deriving practical tests of significance. Thus, charts are given from which may be read off the "probable minimum (multiple) correlation for varying observed correlations and size of sample", and which are based on the theory elaborated by R. A. Fisher as recently as 1928. It may seem natural in the circumstances that the classical researches of Pearson and Yule, who did so much pioneer work in establishing the methods which form the subject matter of the book, should be taken for granted, yet we feel that

they have strong claims to be mentioned alongside those other authors who have perfected the theory for finite samples. The author has, however, performed a useful service in rendering available in yet another textbook the results of the researches of "Student" and R. A. Fisher, and of a number of American workers of whom we suspect that the author himself was the chief inspirer.

After studying the variability of a statistical series and the reliability of statistical results in general, the author goes on to study the relation between two variables, both for the straight-line function and for curvilinear functions, and gives separate chapters to the study of the accuracy of estimate from a sample and the degree of correlation, and to practical methods for working two-variable correlation problems. The same development is followed for determining the way one variable changes when two or more associated variables change, first by using a linear regression equation and later using curvilinear regressions. The relation between a variable and two or more others operating jointly is then gone into, and the author concludes by giving numerous examples of types of problems to which correlation analysis has been applied, and with a valuable last chapter indicating the steps necessary in research work of this kind, and the place in it of statistical analysis.

A clear distinction is kept throughout between the parameters of a population and the estimates that can be made of these parameters from a finite sample from the population. The two most important problems to be solved are, then, (1) that of obtaining the best estimate, and (2) that of measuring the accuracy of the estimate so made. It might have been better had the author refrained from defining *e.g.* the standard deviation of the sample as the square root of the mean of squared deviations from the mean, even though he goes on to give a correction or adjustment for a small number of observations. The more logical course might be to define the estimate directly in terms of the observations of the sample. The simplest case occurs on p. 19, and although the author explains himself fully in the Appendix (p. 372), it might be desirable, to avoid ambiguity, to separate the two ideas of (*a*) estimating the standard deviation, and (*b*) determining the standard deviation of a mean in terms of that of a single observation. The ordinary reader distrusts "adjustments" and "corrections" for which he may not be able to understand the reasons. One has the same feeling about the adjustment of the observed multiple correlation coefficient on p. 177. As far as we know this is the only textbook in which the correction for positive bias of a multiple correlation coefficient is given, and it would perhaps have been more helpful had it been stated that this was done by finding the approximate mean value of R^2 in all possible samples in terms of the population value, and then inverting this expression, replacing mean R^2 by the sample value. Tests of significance are to be applied, of course, to the unadjusted values. The tests are a matter of some considerable interest. R. A. Fisher has given tables from which can be calculated, for a given multiple correlation in the universe, a limiting point, beyond which there is only a 1 in 20 chance of a value of R occurring in samples from that universe. These tables have been charted by our author, who states that they give the probable minimum correlation in the universe. This may seem rather like an appeal to the dangerous doctrine of inverse probability, but, as has been recently shown in another paper by R. A. Fisher, there is a sense in which a definite probability statement can be made about the unknown parameter, which is true irrespective of any assumption as to its *a priori* distribution. It should be pointed out that the author's chart for simple correlation is obtained as a particular case of Fisher's distribution for $n_1 = 1$, and therefore represents the distribution of the correlation coefficient taken as positive, whereas the true distribution extends from -1 to $+1$. In the same way the author's definition of the simple correlation coefficient as the ratio of the standard deviation of the values estimated from the regression equation, to the standard deviation of the original values, leaves the sign undetermined in a way that the more usual definition does not do.

One point which will require justification to the mathematician is the author's use of freehand curves to express trend, when various mathematically-determined curves have failed to fit the data satisfactorily. While the author allows

correctly for the number of degrees of freedom used up in fitting the mathematical curves, he is at a loss to know how many to allow in the case of the freehand curve, and his solution is in the nature of a guess. Thus on p. 133 he judges that a parabolic equation, with three constants, would be adequate to reproduce the freehand curve, and again on p. 223, discussing curvilinear multiple regression, he assumes that third degree parabolae are necessary. But as he has obtained closer agreement between his data and the freehand curve than was possible with mathematically-fitted equations, it seems likely that he has underestimated the number of degrees of freedom to be taken into account, and has therefore overestimated the significance of the fit. How far his term " index of multiple correlation " is needed is a moot point. It might be argued that the ordinary coefficient of multiple correlation was all that was needed, as functions of the 2nd, 3rd, etc., degree in the variables, and product functions representing the effect of two or more variables operating jointly, could be regarded as additional variables to be taken into account with the simple variables in an extended linear correlation study.

The book has been very carefully prepared, and for some time before publication was used as a text in the Graduate School of the U.S. Department of Agriculture. Great care was taken with the proof-reading, and the result is a book free from all except a few minor errors. A useful appendix on methods of computation has been added. J. WISHART.

Solid Geometry. By W. H. MACAULAY. Pp. xii + 303. 14s. 1930. (Camb. Univ. Press.)

We welcome Mr. Macaulay's volume as a useful addition to a strangely small collection of books suitable for teaching Solid Geometry. Within its own sphere it has much to recommend it. Its great merit is that it seeks to be truly geometrical and never becomes—like so many other textbooks on the subject—a mere sequence of algebraic manipulations. A book of this character, suitable for scholarship work in schools and for ordinary degree students in universities has been a long felt want, and the present volume should meet this need admirably. One may regret, however, that the author has not gone a little further and included in the volume enough solid geometry to meet the requirements of all degree students, which after all is not a great deal more than Mr. Macaulay has given us, and would not have made the volume too unwieldy. For instance, we should like some account of the general properties of normals to quadrics, and of the intersections of quadrics, and perhaps the use of moving axes might have been included in the chapter on twisted curves.

This, however, may seem ungrateful to the author, who after all has given us a most valuable book—valuable not only because the solid geometry which it contains is treated in a most pleasing fashion, but because it includes some chapters on topics which are not often dealt with in convenient textbooks, but which form a most valuable part of any course in mathematics. In addition to an account of all the spherical trigonometry which the average student requires it contains a useful if elementary account of the geometrical theories of strain and of vector distributions. These chapters will be of great value to students beginning courses in which such ideas are of frequent occurrence.

A detailed examination of the volume reveals little to which exception can be taken. We personally disagree with the author's choice of sign for the torsion of a curve, but this is a matter of opinion in which the author is entitled to his own view. But certainly a student with a sense of geometry will appreciate this new book and will find in it an inspiration which will carry him on to fresh fields. W. V. D. H.

London University Guide. 1931-1932. Pp. 217. 2s. 6d. (Burlington House, Cambridge.)

This publication re-appears after a gap from 1927. It gives in a compact form a great deal of information concerning the examinations of London University and the University Correspondence College, which should be useful, particularly to private students.

THE LIBRARY.

160 CASTLE HILL, READING.

THE Librarian reports gifts as follows:

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From the Committee for their republication:

J. W. GIBBS — Collected Works - - - - - - - - 1928
(2 vols.)

From Miss **M. H. Greaves,** a collection of back numbers of the *Gazette*.

REPORT ON MATHEMATICS IN ENTRANCE SCHOLARSHIP EXAMINATIONS.

THE General Teaching Committee has recently conducted an investigation into the papers set at about fifty Public Schools, and it appears that the vast majority of the papers are on the lines advocated in the *Report on Mathematics in Entrance Scholarship Examinations*. This may be of interest to teachers at Preparatory Schools, since it makes it unnecessary for them to cover a large syllabus with their prospective scholars.

CORRIGENDUM.

Note 991. [A. 3. k.] *The Numerical Solution of Cubic Equations by Circular Functions.* Vol. XV, p. 391 (May, 1931).

The last line of my note on Cubic Equations should read:

"By Horner's method I obtain

$$y_1 = -2{\cdot}8216401644\ldots".$$

It is odd that in this case one gets a result correct to seven figures with the use of only seven-figure logarithms. A. S. PERCIVAL.

CORRESPONDENCE.

GAMBLING.

To the Editor of the *Mathematical Gazette.*

SIR,—By the courtesy of a member of the Mathematical Association I have been enabled to read the report of the discussion on Gambling in your issue of March 1931. Mr. W. Hope-Jones quoted my definition of Gambling, namely, that it is "An agreement between two parties by which the transfer of something of value is made wholly dependent on chance in such a way that the whole loss of one party is the whole gain of the other," and said that it broke down as applying to insurance. He is, of course, wrong. It does not apply to insurance. In a properly conducted insurance there is neither loss nor gain. The merchant who insures his goods loses nothing if he never has to make a claim. During the time that he is covered he enjoys the usage of a perfectly real and marketable, though not a tangible, good, namely, security. That it is both real and marketable is proved by the fact that his banker will allow him a larger overdraft on the security of insured goods than on uninsured ones. If a merchant has to make a claim he enjoys no gain. What he receives from the Insurance Office merely balances his loss from fire. If he is found to have made a profit he is likely to have a very unpleasant interview with the Fire Assessor.

It is curious how otherwise clear-thinking men always go wrong on the question of the relation of gambling to insurance.—Yours,

PETER GREEN, Canon of Manchester.

To the Editor of the *Mathematical Gazette.*

SIR,—As I make no pretence of being able myself to produce a definition of Gambling, (or of anything else), in which no holes can be picked, I have no intention of quarrelling with Canon Green's definition as he enunciates it here. In the original form in which I quoted it from his book, *Betting and Gambling* (1924 edition), "an uncertain event" was substituted for "chance", and the saving words "whole" and "wholly", which in my opinion add so much to the strength of the modern form of it, were omitted. I am sorry if I have unwittingly quoted an out-of-date form of Canon Green's words when a better form was already in print; but he will agree with me that it is impossible to buy and read every edition of every book.

Lest I should be included in the classification of "otherwise clear-thinking men who go wrong on the question of the relation of gambling to insurance", may I add that on this subject I am in complete agreement with Canon Green. It is only on the limitations of a verbal definition that I have ventured any criticism.—Yours,

W. HOPE-JONES.

JAMES MAURICE WILSON

1836–1931

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